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OF THE
ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

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NOTICES

Election of Members

The following members were elected at a meeting of Council held on January 16th, 1923 :—

Students.—H. L. Calder, G. S. Mills.

Air Conference Representatives

The Council, in response to an invitation from the Air Ministry, have nominated the following members to represent the Society at the Third Air Conference to be held at the Guildhall on February 6th and 7th, 1923 :—

Mr. Griffith Brewer, F.R.Ae.S.
Mr. Robert Bruce, A.F.R.Ae.S.
Mr. W. D. Douglas, A.F.R.Ae.S.
Captain W. Scott Farren, A.F.R.Ae.S.
Mr. H. Glauert, M.A., A.F.R.Ae.S.
Lieutenant-Colonel S. Heckstall-Smith, F.R.Ae.S.
Captain G. T. R. Hill, M.C., A.F.R.Ae.S.
Professor C. F. Jenkin, C.B.E., F.R.Ae.S.
Mr. J. L. Pritchard, F.R.Ae.S.
Major-General Sir R. M. Ruck, K.B.E., C.B., C.M.G.
Mr. H. L. Stevens, A.F.R.Ae.S.
Dr. H. C. Watts, F.R.Ae.S.

Aeronautical Classics

It has been suggested that a further series of monographs on the work of various pioneers should be embarked upon, to provide when completed a second volume to "The Aeronautical Classics." The Publications Committee consider that this is a very desirable work if the Society's finances permit of it being undertaken. As a preliminary it was decided to invite members to indicate their willingness to subscribe to such a series in order that some idea may be gained of the probable call upon the Society's funds to meet the balance of the cost. Among those with whose work it is proposed to deal (covering the period 1883-1903) are Laurence Hargrave, Otto Lilienthal, S. P. Langley, Octave Chanute, and Sir Hiram Maxim, and it is anticipated that the price would be about 3s. 6d. to 5s. od. per volume, according to the number of subscriptions received. Members willing to undertake to subscribe to the series are asked to communicate their intention to the Secretary.

It may be recalled that "The Aeronautical Classics" were published during 1910, and comprised volumes on Sir George Cayley, F. H. Wenham, Thomas Walker, Francesco Lana, Percy Pilcher and F. J. Stringfellow, and G. A. Borelli.

Students' Section

Arrangements have been made to hold a smoking concert for students and their friends at the Engineers' Club, Coventry Street, London, W.1., at 6.45 for 7 p.m., on Friday, February 16th. Tickets, price 1/6 each, may be obtained from the Secretary. Early application is advisable.

R38 Memorial Research Fund

The following further donations towards this fund have been received :—

	£	s.	d.
Officers of the Staff, British Aviation Mission, Japan	20	0	0
Mrs. Little	8	18	9
Officers R.A.F., Peshawar	7	15	2
Lieutenant-Commander White, U.S.N....	5	0	0
E. G. Walker	2	5	0
Further Interest on Deposit Account	9	18	4
Previously announced 1211	2	8	
Total to date	£1264	19	11

The following entries for the R.38 Memorial Prize have been received. Entrants are reminded that their papers, together with the signed declaration required by the regulations, must reach the Secretary not later than March 31st :—

Commander F. L. M. Boothby.
Captain Elbridge Colby.
A. P. Cole.
Wing Commander J. N. Fletcher.
Commander J. C. Hunsaker, C. P. Burgess, S. Truscott (*Joint Authors*).
Robert Jones.
Norman Meadowcroft.
Professor Dr. T. v. Karman.
Major G. H. Scott, Lieutenant-Colonel V. C. Richmond (*Joint Authors*).
R. H. Upson.
Ing. Rodolfo Verduzio.
Dr. Karl Wegener, Dr. Karl Schneider (*Joint Authors*).

Arrangements for the Month

Feb. 1, 5.30 p.m. Royal Society of Arts. Mr. G. S. Baker, O.B.E., late R.C.N.C., M.I.N.A., "Ten Years' Testing of Model Seaplanes."

,, 8, 7.0 p.m. *Students' Section.*—Society's Library. Mr. W. L. Le Page, "Experimental Methods in Aerodynamics." Mr. A. Fage, F.R.Ae.S., in the chair.

,, 15, 5.30 p.m. Royal Society of Arts. Wing-Commander T. R. Cave-Browne-Cave, "The Practical Aspects of the Seaplane."

,, 20, 5.0 p.m. Council Meeting.

,, 22, 7.0 p.m. *Students' Section.*—Society's Library. Mr. T. A. Kirkup, "The High Lift Wing." Major O. T. Gnosspelius, A.F.R.Ae.S., in the chair.

Mar. 1, 5.30 p.m. Royal Society of Arts. Major F. M. Green, F.R.Ae.S., "Helicopters."

W. Lockwood Marsh, *Secretary.*

PROCEEDINGS

THIRD MEETING, 59TH SESSION

An ordinary general meeting was held at the Royal United Service Institution, on Thursday, November 2nd, Prof. Bairstow, Chairman, in the chair.

The CHAIRMAN, in opening the meeting, said that his opening remarks would be confined strictly to the chairman's preliminary business, viz., that of introducing Major Low to those present, or rather to those who did not know him. Those who did know Major Low and had an advance copy of his paper would realise that the paper was typical of the man, very interesting and very contentious. (Laughter.) That was one reason why he himself wished to keep his present remarks short, because he hoped to get a later opportunity of speaking on the subject. Major Low had been connected with aeronautics through the whole of what might be called the modern period of development, that was from a little before 1910 to the present day. In the early period of that time he was designing aircraft and applying his mathematical faculties to the general problem. In particular, he read a paper before the Society in 1913 in which he gave mathematical extensions of the Drzewiecki theory of airscrews. That paper was in the Journal and gave rise to valuable discussion at the time. In the present paper Major Low proposed to give a summary of the theories as presented by other people with his own critical faculties exerted in the selection of material and its presentation. There was one other point he (the Chairman) wished to mention now, although it really had not anything to do with Major Low's paper, except for the fact that he mentioned Prandtl's theory. Major Low had quite properly kept his remarks very general. There were very few mathematical symbols throughout his exposition and consequently the matter was easier to follow. The Council of the Society had felt for some time that opportunities might quite well be afforded to those members who were really expert in a subject, to meet and discuss points of great difficulty. On these occasions it would not be necessary that the member speaking should make himself easily intelligible and the assumption could be made that the rest of the audience had a great deal more knowledge than they were likely to possess at the moment. There might be occasions on which different speakers could attack a given problem from so many points of view that they never came into contact, and although contact might produce fireworks and a good deal of interest, that did not necessarily make any difference to the importance of discussing a new subject. Amongst the new subjects of aeronautical interest it seemed to the Council that the Prandtl theory was one of the best for discussion and Mr. Glauert, of the Royal Aircraft Factory, had undertaken to prepare a paper and initiate a discussion. Amongst the members of the Society were quite a number who could speak with first hand knowledge of the subject, and by that means the value of the Prandtl theory, its application to practice, etc., would be more widely appreciated and a more or less organised body of thought formed. In these discussions they would not be limited as to time; the sittings must not be unduly prolonged individually, but they could be adjourned as necessary. It was proposed to hold the meetings in the rooms of the Society. The Chairman then called on Major Low to read his paper.

Major LOW, before reading his paper, said: I am very much touched by the way in which the Chairman has returned the soft answer in advance, but I shall wait until I have heard his second speech before I say more about it, and shall proceed with the paper at once.

REVIEW OF AIRSCREW THEORIES

BY MAJOR A. R. LOW, FELLOW.

The early ideas of a marine screw seem to have been based to some extent on the invention of Archimedes for raising water by turning a helicoidal pipe or channel about its axis inclined at a less angle than that between the helix and a plane perpendicular to the axis.

At other times the very imperfect analogy with a screw turning in a solid nut seems to prevail. The introduction of the notion of slip allowed of a fairly complete definition of the kinematical state of the screw, without attempting

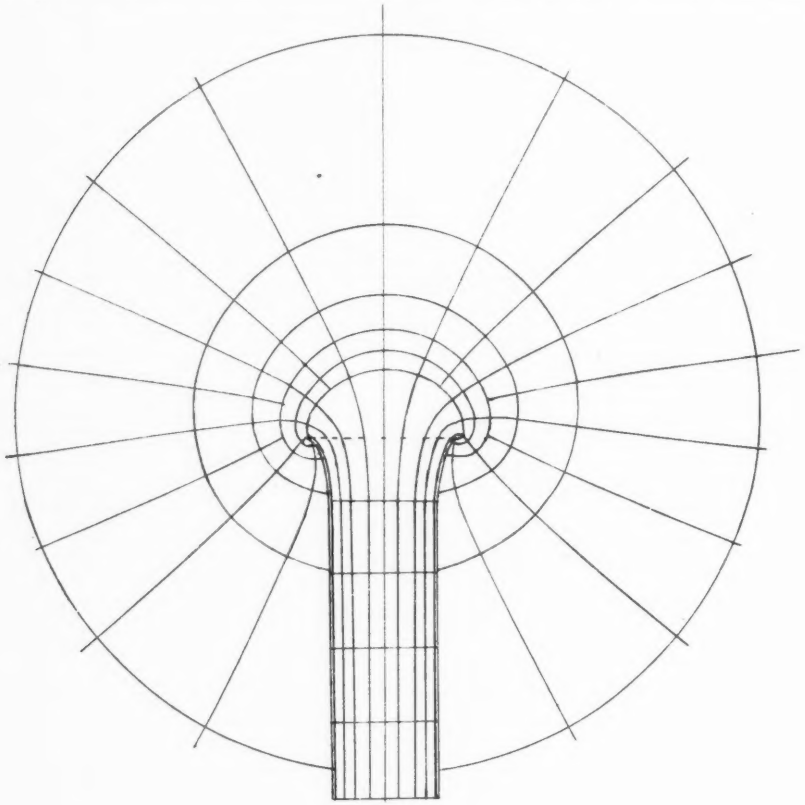


FIG. 1.

to describe the motion of the surrounding fluid.* Early water screws (1810-1840) had one, two or more helicoidal surfaces of a complete turn or more, which were in fact the blades of modern practice with enormously exaggerated chords.

Passing over the claims of the Chinese, the suggestions of Hooke, and

* The French word *régime* is rendered by the word "state," qualified where necessary for clearness by appropriate adjectives.

the unsuccessful attempts of British, French and American inventors, we find one Smith with a vessel of over two hundred tons arriving, about 1840, at the decisive result that a two-bladed screw with chords one-sixth of a complete turn gave far better results than the Archimedean form.

H.M.S. "Rattler" (1843) achieved some ten knots, and two years later the "Great Britain" crossed the Atlantic for the first time under screw propulsion.

From that date there has been an enormous literature, patent and technical, on the subject. Rankine was one of the first to give a precise hydrodynamical

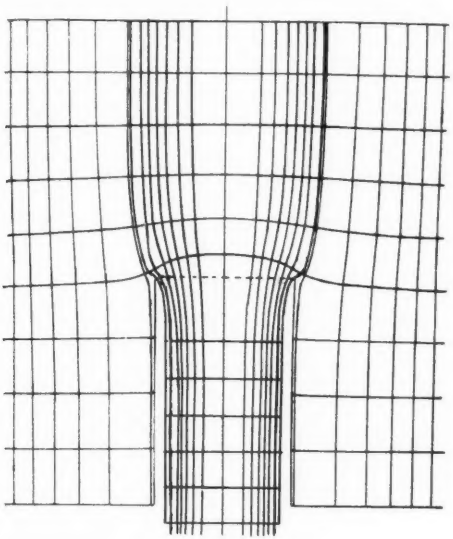


FIG. 2.

form to the theory of water screws (I.N.A., 1865). William Froude, Coriolis and others have claims to joint honours.

In Rankine's theory the screw was regarded as an actuator disc imparting momentum to a column of water drawn from the front and thrust to the rear.

Mr. R. E. Froude, son of William Froude, gave a more modern form to the theory by assuming that each element of blade put in motion the column of water standing on it axially (I.N.A., 1899).

Sir George Greenhill showed later that if rotational velocity only were imparted by the blades to the column, this would account for axial thrust on the screw. As we now know very clearly, both axial and rotational velocities are imparted to the column of water or air, the theory being incomplete without the consideration of both components.

Figs. 1-3 show conventionally the flow through the screw disc acting as an actuator, the column of water thrust back remaining a circular cylinder for an indefinite distance. The kinematic states are those of 100 per cent. slip or standstill, 50 per cent. slip and 20 per cent. slip.*

* For the nature of the actuator and for accurately drawn diagrams of the flow, see H. Kimmel, Z.f.F.u.M., 24th February, 1912, p. 53.

An interesting discussion took place (I.N.A., 1911, p. 139) between R. E. Froude and Professor Sir J. B. Henderson (representing different Admiralty Departments). The point in dispute was really the conservation of energy in the surrounding fluid. With a double source in the shape of a flat disc not in motion relatively to the undisturbed fluid at a distance there is a finite amount of kinetic energy (Lamb, p. 139, equ. 5) which is completely conserved in a perfect fluid. If the disc is in motion relatively to the fluid, if we suppose its strength unaltered the energy is unaltered. But if the strength vanishes for

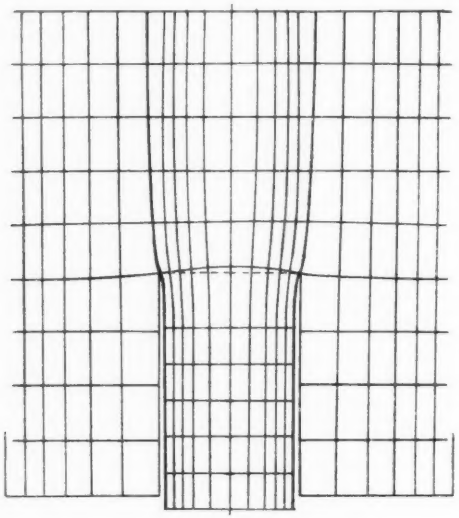


FIG. 3

a given axial velocity V_0 and if it is proportional to $V_0 - V$ for values of V not exceeding $V_0/3$, say (30 per cent. slip), then the energy is proportional to $(V_0 - V)^2$ or $V_0^2\,(1 - s)^2$ where s is the fractional slip.

In a perfect fluid, then, for any state (given strength and slip) there is complete conservation of energy once steady motion is established.

Actually there is very complete dissipation of energy, except in small enclosed basins or chambers.

If the speaker has not misapprehended the case, Henderson had the better in theoretical discussion and Froude in his conclusions.

The Rankine-Froude theory, with certain corrections for hull interference, has been of service in laying down limits for thrust energy and efficiency, but it depends for its application on empirical coefficients expressing mean velocities and momenta. In principle, it is an application of Bernoulli's theorem connecting velocities and pressures.

The columnar nature of the outflow tacitly accepted above is, in fact, well established experimentally. Its mechanism will be explained by Prandtl's theory.*

* It is of interest to refer to the R.A.E. experiments on flow about an airscrew whose tip speed exceeds that of sound. In this state the axial columnar outflow disappears and radial outflow is set up.

The Aerofoil Analogy

The next method to be described is now generally known as Drzewiecki's, and indeed this scientific engineer has overwhelming claims on the score both of priority of idea and of practical development (1892).

The main idea is now so familiar that we are apt to under-estimate the insight shown by Drzewiecki at a time when there were no precise data on lift and drag coefficients, and when the very idea was vague.

He considers that cylindrical sheets, contained by co-axial tubes of revolution, flow past blade elements in a manner kinematically nearly identical with the flow of plane sheets across aerofoil elements. Each such sheet—plane or figure of revolution—he supposed to be kept in its steady position by the adjacent sheets on each side. Thus each element had an invariant reaction, for a given kinematical state, with invariant components parallel and perpendicular to the element of the path relatively to still air. Assuming, further, that the reactions were proportional to density and path velocity square, he obtained coefficients of reaction closely analogous to lift and drag coefficients of an aerofoil. Finally, assuming a simple blade shape (constant chord) and integrating over the blade, he obtained expression for thrust and torque.

Applying the well-known theorem of the mean, he replaced the variable coefficients inside by mean values outside the sign of integration. Comparing these expressions with observed thrust and torque, he obtained experimental values for mean lift and drag coefficients. Having accumulated a sufficient number of values for different incidences, etc., he was able to design new airscrews with different geometrical pitches, blade lengths, for various working states.

Lanchester (1906) gives a blade element theory which in many respects is identical with Drzewiecki's, but the speaker always failed to establish a routine for numerical computation from his work. Only when Drzewiecki's theory came into his hands, in a brochure, "La Technique de l'Hélice Aérienne" (Gaston Camus, Paris, 1909) and when Eiffel's wing tests were published was it possible to make calculations which had a fairly close relation to practice.

The present writer's paper read to this Society was one of the first systematic accounts in English. (A paper by Bolas, dated March and June, 1912, R. and M. No. 65, may have been published earlier, and gives a useful summary with examples.) In the discussion Major Bramwell offered strong criticisms on the ground that there was no experimental evidence! He stated that it was possible to design a screw without any theory at all, and quoted (with reserve indeed) the astonishing statement that anything that looks like an airscrew has an efficiency of about 70 per cent.

Time sometimes brings its revenge, and half a year later Mr. Bramwell read a paper before this Society on his experimental work at the N.P.L., which put Drzewiecki's method on an independent experimental basis, so far as regards any ordinary working state occurring in flight.*

Apparently Major Bramwell offered the criticisms referred to, although they were already destroyed by his own work. Possibly the A.C.A.'s method of dating explains the matter. Open publication is the only real test of work, and this was often from one to two years after the dates borne by the papers.)

The geometry or kinematics of the airscrew is simple enough in principle, but it leads to fairly complicated algebra. The speaker showed how to express

* The following dates are interesting :—
Low, Drzewiecki's Theory, Journal, October, 1913.
Bramwell, Propellers, Journal, April, 1914.
Bramwell, A.C.A., R. and M. 82, March, 1913 (!).

Drzewiecki's integrals by means of hyperbolic functions, not only for uniform chord but for a variety of forms.

Mr. Riach has done excellent work in developing the analytical treatment (Airscrews, 1916, in "Aeronautics," and in this Journal).

Soreau's chapter on integration (l'Hélice Propulsive, Soc. des Ing. Civ., 1911, p. 149) is admirable.

Of course, the known mathematical devices of graphical and statistical integration enable all difficulties of analysis to be avoided, but it is a great advantage to have a number of forms worked out analytically and tabulated.

Flow Round Airscrew Blades

It is evident on the most cursory observation that the average flow round an airscrew is well represented by Figs. 1-3 if a rotary motion is given to the columnar wake or outflow.

There is a tacit assumption in taking coefficients from aerofoil tests and applying them directly to blade elements that the relative flow of air is not sensibly disturbed until it comes under the direct influence of the blade, just as it is undisturbed save by the aerofoil in a monoplane test. But if aerofoils are placed in tandem, the rear foil is sensibly affected by the leading foil.

If now a multiple cylindrical section be developed on a plane, the blade sections in a normal working state of the screw will appear as a multiplane with high stagger. There is, therefore, to be looked for, a mutual interference. This was suggested by Bolas, by the writer, and by numerous German authors. Lanchester gave a detailed statement (Aerodynamics) many years earlier.

The claim made on behalf of Messrs. Wood and Glauert of the R.A.E. that they were the first to point this out is based on ignorance of the literature of the subject. They have, however, the credit of having put into practice a multiplane test with a view to measuring the mutual interference, R. and M. 639.

The speaker considered this at the time as the most promising development yet put in hand. It is, however, superseded by Prandtl's theory, though it may be a highly useful check.

Another view of interference is based on the observed inflow. At the N.P.L. much work has been done on measuring the root mean square velocity round annular spaces between concentric circles.

In Froude's actuator theory, where the actuator is a thin disc, it is shown that in a perfect fluid half the acceleration takes place before the disc and half behind.

Accordingly, it has been proposed to take half the total increase in mean velocity as altering the true angle of incidence of the blade elements. The results, far from correcting the simple aerofoil analogy, make it more discordant with observed thrust and torque. This leads to the introduction of empirical coefficients to correct the correction. (Full discussion and A.C.A. references will be found in Bairstow's "Applied Aerodynamics," Chapter VI., and in Fage's "Airscrews." Their foreign references are inadequate.)

Dr. Watts has given out of his very wide test experience the view that the uncorrected Drzewiecki method gives closer results than the inflow correction (*cf.* Screw Propellers, Chapter VII., Appendix). Further light was thrown on the whole subject by Drzewiecki's crucial experiment (Théorie Générale des Hélices Aériennes, 1920, p. 128). With a single blade, measuring instantaneous values of the velocity instead of the root mean square velocity round a circle, he showed that it was pulsating, rising sharply to a peak and falling less steeply nearly to the undisturbed velocity.

The immediate inference is that by far the greater part of the inflow near a blade is due to the blade itself. This, as will be seen, is substantially confirmed by Prandtl's theory.

The stroboscopic experiment was brought forward by the speaker in a discussion on Sir Richard Glazebrook's lecture (26th May, 1920; Journal, September, 1920), Dr. Stanton's work having been quoted as the latest advance in airscrew theory. The speaker is not deterred by the voice of authority from saying frankly that the application of Froude's ideal velocity ($V_0 + V/2$) at the blade finds no justification in the actual phenomena. Dr. Stanton's work, of course, has its place quite apart from this application. But it must be emphasised that Drzewiecki's stroboscopic method is far more illuminating, just as the oscillograph reveals much that the ordinary ammeter and voltmeter fail to record. Dr. Watts has since given a more detailed account and has drawn similar conclusions (Journal, July, 1920); but some amendment is required to his method of superposing velocities.

Professor Bothezat, on the contrary, opposes these conclusions (Journal, November, 1920, p. 597). His argument is ingenious. But for the true inflow there would be a diminution of the undisturbed flow at angular distance π to compensate the increased flow near the blade. It seems to the speaker that undisturbed flow at a distance from the blade is the state that matters.

Mr. Riach (Journal, September, 1920, p. 527) evidently takes the same general view as Dr. Watts and the speaker, and develops it at length in "Aeronautics." If mild criticism may be offered, his physical ideas are rather submerged in symbols, *apparent rari nantes in gurgite.* His paper (Journal, February, 1922) works out the "cascade" correction as a logical extension of Drzewiecki's method.

The Lanchester-Prandtl Theory

All these attempts to form physical theories are brought into perspective by the brilliant developments of the last twenty years, principally in Germany.

In this country Mr. Lanchester has given remarkably complete descriptive accounts of the theory here exposed (Aerodynamics, 1907). Unfortunately, research in this country has completely failed to respond to the stimulus. This may be partly explained by the absence of any account as to the manner in which Lanchester arrived at his conclusions, and of references to the work of others, from whom he may be supposed to have drawn suggestions or experimental evidence. Further, all attempts to base a routine of calculation on his exposition have failed the speaker and such of his friends as he has consulted. This is now explained by a glance at the immense amount of work, in several highly specialised directions, required to put the new theories on a quantitative basis.

Mr. Lanchester does indeed claim to have made such computations with satisfactory results, but in view of the rudimentary state of the theory at the time their value cannot have been very great in the predetermination of an airscrew.

None the less, Lanchester's work shows remarkable insight into the physics of a problem that baffled the scientists of last century. Had our physicists followed up his ideas, this country might have shared in the work. Although not till recently honoured in his own country, Lanchester has had very full recognition in Germany, unlike Bryan, who is generally ignored. In Joukowsky's words, "Lanchester's distinguished service is the elucidation of the transition from plates of infinite span, which render the space containing the fluid multiply connected, to finite plates in simply connected space" (Z.f.F.u.M., 1920, p. 282). Compare this with Reissner's reproof to German writers, "Bryan's

highly distinguished service in first (1904) putting the problem of aeroplane stability in complete mathematical form should not be ignored in citing names" (Jahrbuch d. Wiss. Gesell. f. Luftfahrt, 1915/15, p. 141).

Kutta gave the new theory its first impulse by showing how a circle with streamlines past it and circulation round it could be conformally represented by an arc of a circle likewise with flow past it and circulation round it.

On combining the two, a reaction is obtained at right angles to the undisturbed uniform flow (Ill. Aer. Mitteilungen, 1902).

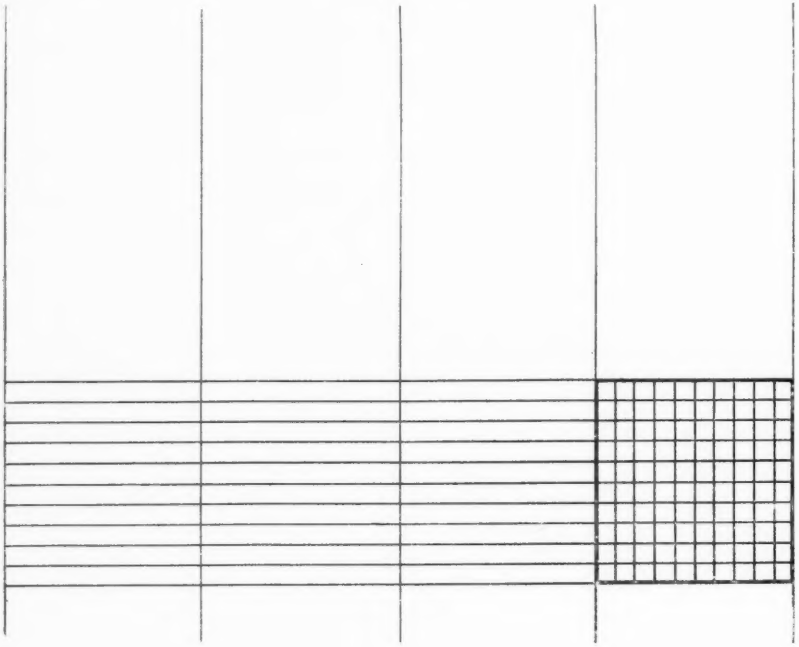


FIG. 4.

Joukowsky extended the transformation to wing-like profiles or sections, now known as Joukowsky profiles (Bull. de Koutchino, 1906; Aérodynamique, Paris, 1920).

Before describing the results obtained by Betz, Munk, Trefftz, Blasius, v. Mises, Kármán and others, as well as by Prandtl himself, the speaker proposes to give a few of the simplest cases of flow of a perfect incompressible fluid in a form which can be followed by engineers without specialised mathematical training. Those who are more fortunate will have patience for a few minutes.

The simplest case of possible motion of a perfect incompressible fluid is that in which every particle has the same uniform velocity. Fig. 4 shows the plane right section of the sheets of flow. Now consider Fig. 5; the rays from the centre are equally spaced and the radii of the circles are in geometrical progression. By choosing the ratio of the radii as exp. (θ) where θ is the angle between rays we get each small division as a square to the second order of small quantities.

Thus to each small square in Fig. 4 corresponds a small figure nearly a square in Fig. 5.*

By taking the subdivision fine enough we get the meshes as nearly square as necessary, and by mapping with the network of small squares as lines of reference we get small corresponding areas in each system, as similar figures.

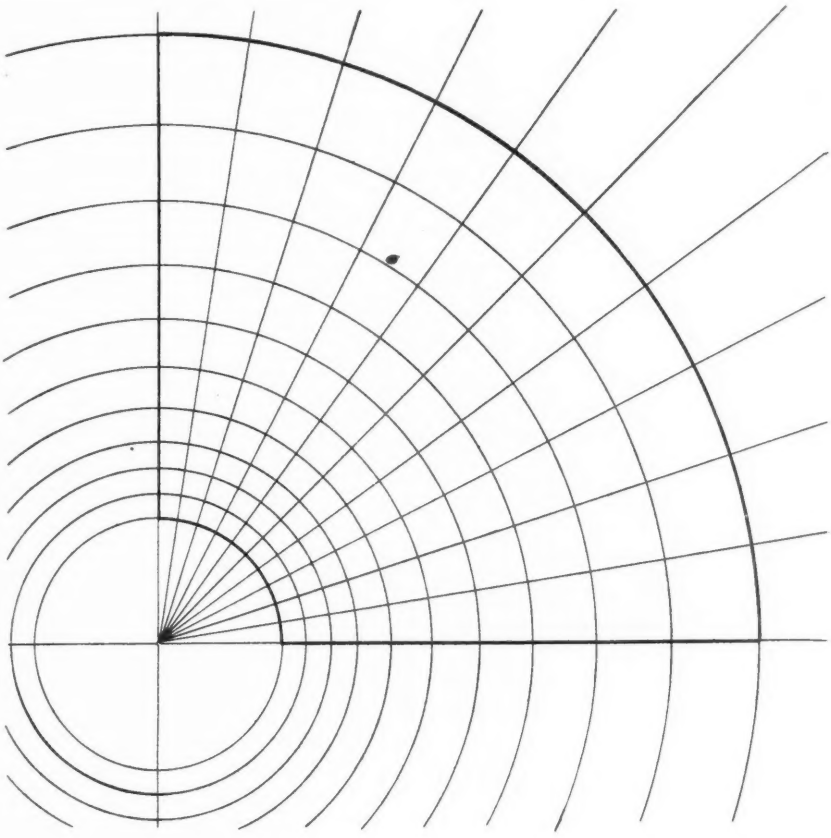


FIG. 5.

To the first quadrant in Fig. 5 corresponds the right-hand vertical strip in Fig. 4; to the whole circle corresponds the whole of Fig. 4 between the external vertical lines.

If we go round the circle many times anti-clockwise or clockwise, we get a series of strips left or right. The circle of very large radius corresponds to a straight line parallel to the horizontal at a great distance above, and the circle of very small radius to a similar straight line very far below.

* We have exp. $(\theta)=1+\theta+\theta^2/2+\ \ldots$ and by choosing θ small we get the ratio of the radii approximately $(1+\theta)$ while the sides are in the ratios $1 : 1+\theta/2 : 1+\theta/2 : 1+\theta$.

The simplest possible case of flow of a perfect incompressible fluid is illustrated in two ways by Fig. 4. We may either consider the vertical lines as lines of flow, and the horizontal lines of no flow, or *vice versa*.

A useful interpretation of lines of no flow in the analogous figures of electrostatics is as lines of equal potential, no work being done when a small charge is carried along an equipotential line. A similar function is obtained in this case as a velocity potential.

Let A and B be any two points (x_1, y_1) (x_2, y_2) in Fig. 4, joined by a curve s. Divide the curve into small arcs ds and project the velocity at mid arc on the tangent, and we get the product

$$v \sin\theta \, ds = v dy \} \text{ case 1.}$$
$$v \cos\theta \, ds = v dx \} \text{ case 2.}$$

Since v is constant, we get, in vector notation,

$$\int \mathbf{v} \, d\mathbf{s} = v \ (y_2 - y_1) \text{ in case 1, } = v \ (x_2 - x_1) \text{ in case 2.}$$

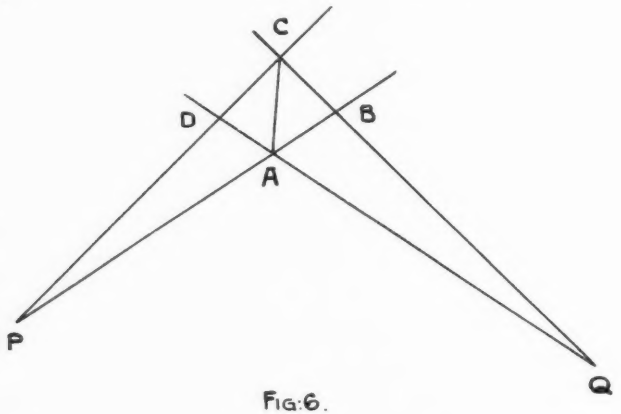


FIG. 6.

Hence the velocity potential difference between two points in either case depends only on the difference of potential between the equipotential lines they are on, and not on the path joining them. It may be denoted by $P_B - P_A$.

An advantage of knowing the potential function is that the velocity in any direction is found by evaluating dP/ds.

The lines parallel to the x and y axes are therefore lines of equal velocity potential in cases 1 and 2 respectively. It follows at once that integration round any closed path APBQA is zero, for the integral along APB is equal to the integral along AQB. Defining this integral taken round a closed path as the circulation, we find as a characteristic of this type of flow that the circulation round a closed path is zero, just as the work round an electrostatic path is zero.

Looking now at Fig. 5 there are again two interpretations, (1) the rays are lines of flow and the circles are lines of no flow. In case 1 the velocity is clearly inversely as the arc between rays, that is, inversely as the radius. Taking the circulation round any square, it is obviously zero by symmetry, the integral along one side being cancelled by the equal and opposite flow round the other.

In the other case (2) the circles are lines of flow by symmetry constant round a circle, and the rays are lines of no flow. The circulation is $v_1 \cdot r_1 \cdot d\theta$

$-v_2 \cdot r_2 \cdot d\theta$, and if this is zero $v_1 \cdot r_1 = v_2 \cdot r_2$, *i.e.*, the velocity, as in the last case, is inversely as the radius.

It can be shown that this condition is sufficient to make the circulation in any circuit zero, provided it does not include the origin. If, however, we integrate once round a circle of flow we get the potential difference $2\pi v r$ where r is any radius and v the corresponding velocity.

But $v r = v_1 r_1 = v_2 r_2 \ . \ . \ . \ = \kappa$ say, and κ is defined as the strength of the circulation. This is Helmholtz's vortex motion, and it is irrotational; in other words, a velocity potential exists, except at the centre.

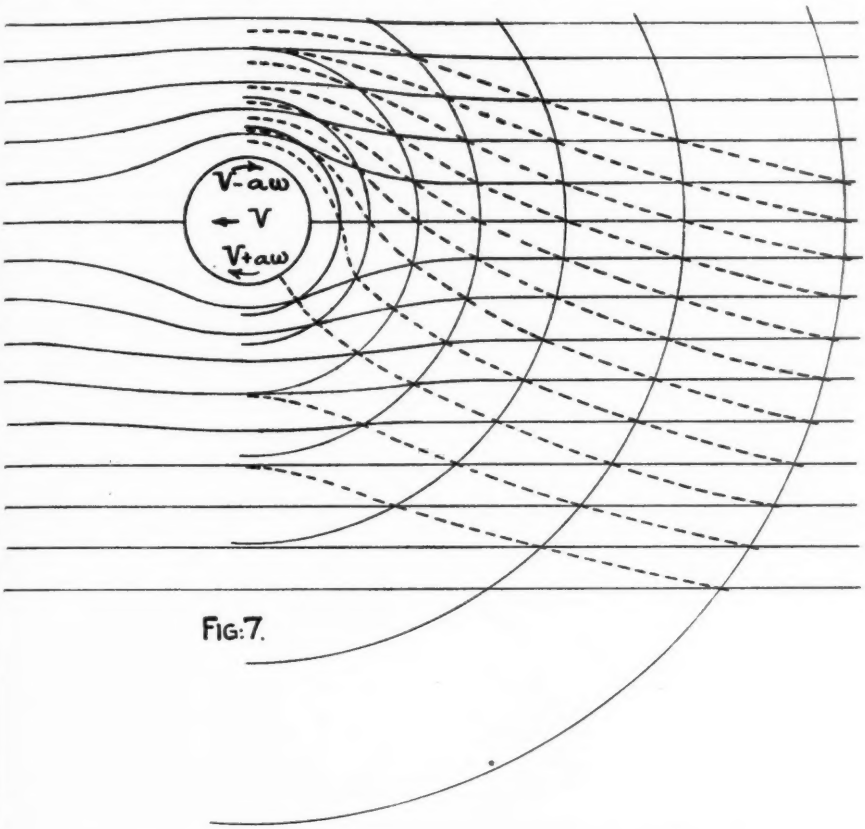
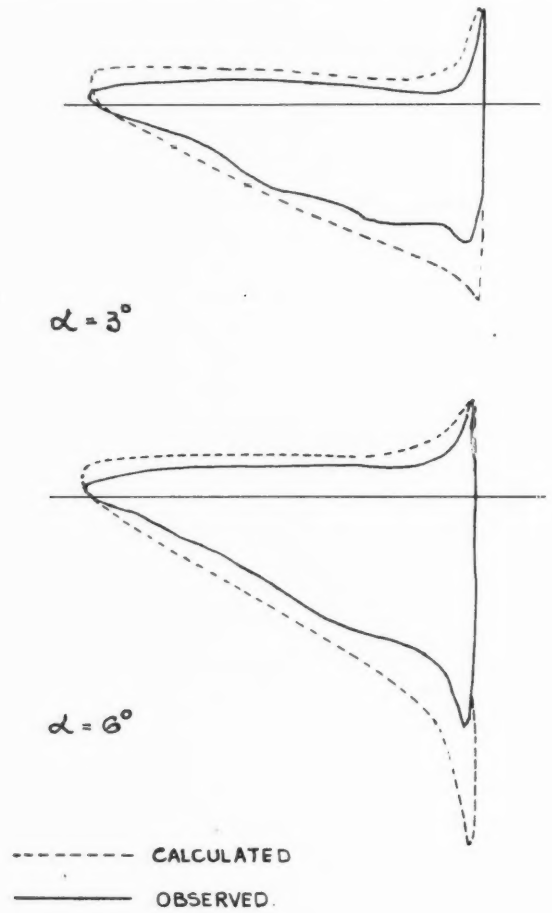


FIG. 7.

There are certain restrictions in interpreting these results. At the centre of Fig. 5 the conformal nature of the representation fails completely, and in correspondence with this failure it is physically impossible to have a pipe of infinitely small diameter delivering fluid at infinite velocity and with infinite energy. It is likewise impossible for the vortex motion to continue up to the centre, where the motion would again require infinite velocity and energy. In fact, a core forms which is void or filled with air or fluid rotating uniformly or in some other physically possible way.

If two such irrotational or potential flows are superposed the resultant flow may be found graphically (Clerk Maxwell, El. and Magn., Vol. 1., Chap. VIII.).

Let P Q (Fig. 6) be two sources, and let the rays in pairs enclose channels or tubes of equal flow, then the flow across A B from P is equal and opposite to the flow from Q, so that the joint flow is zero, and A B is a line of flow in the combined system. Applying this from point to point we trace out the new lines of flow. It is possible in this way to build up graphically more and more elaborate cases from simpler cases. We may also superpose



FIGS. 8 AND 9. DISTRIBUTION OF LIFT.

systems of equipotential lines, for the velocities along any path are additive; hence the differential coefficients in every direction are additive, and the potential functions themselves are additive.

As engineers, we may accept worked-out cases from any source, *e.g.*, from analysis or from Hele Shaw's experimental determinations (I.N.A., 1898).

An important case is found in Lamb's Hydrodynamics, p. 75. Superposing

the vortex motion of Fig. 5 we get Fig. 7. (It should be noted that this is not identical with the usual examples in electrostatics and electromagnetism.) The flow in Fig. 7 is the basis of the transformations of Kutta and Joukowsky. There is bunching of the lines of flow, showing increase of velocity and diminution of pressure above the circle, and thinning, showing decrease of velocity and increase of pressure below.

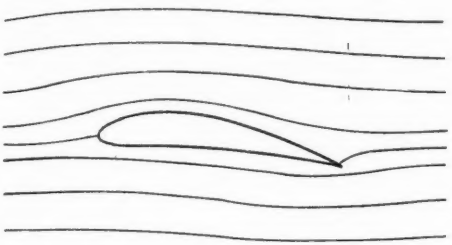


FIG: 10

This follows from Bernoulli's relation between pressure and velocity (ignoring the gravity field).

$$p_0 - p = \rho v^2/2 - \rho v_0^2/2$$

There is therefore a resultant pressure vertically upwards, the flow being horizontal at a distance. This reaction R is given by the equation $R = \rho \kappa U$ (Lamb, p. 76), where ρ is the density, κ the strength of the circulation, and U the undisturbed velocity at a distance. This holds for any boundary, as is seen by the principle that at a distance large in comparison with the boundary the lines of flow of the circulation are nearly circles with centre at the centre of position of the system.

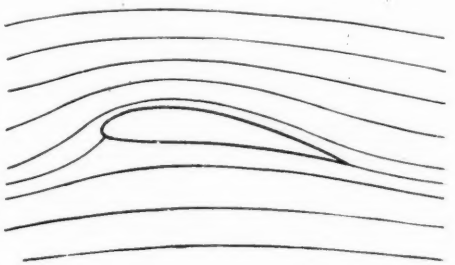


FIG: 11.

This useful principle also holds for lines of flow at a distance from any combination of sources. (See Lamb, p. 666, sect. 371-2 and notes, giving general references on the subject-matter of this paper).

Examples of circulation combined with linear flow are common in our playing fields. Rayleigh found a problem for investigation in the flight of a cut tennis ball (Mess. of Math., 1878) and Tait in the flight of a golf ball,

whose flight with under-spin, over-spin, right spin and left spin are only too familiar in the last three cases as topped, sliced and pulled drives, the first giving the easy and prolonged flight of a correct stroke.

The existence of circulation round a ball with spin and forward motion follows at once if we admit frictional reaction between the surface and the air in its neighbourhood, increasing with the relative velocity. The mechanism is almost intuitively established. We are, however, making the tacit assumption that the fluid acts as viscous near the solid boundary and as inviscid at a distance. This is, indeed, the basis of recent advances in hydrodynamics, and it has led, as will be seen, to a mechanism of flight which brings physical theory into something like accord with observed phenomena.

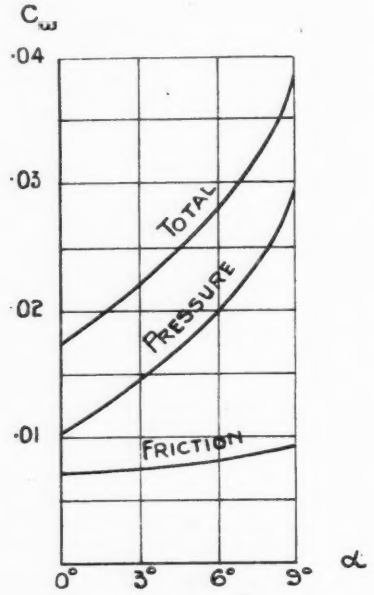


FIG. 12.

To return to the work of Kutta already mentioned, the strength of circulation was so chosen that one of the two points of infinite velocity at nose and tail—that of the tail—was eliminated by causing the streamlines there to run parallel to the tangent.

Joukowsky extended the transformation of the circle to wing-like profiles (Fig. 10) with flow past it. Superimposing circulation he obtained (Fig. 11) with no point of infinite velocity, the strength of circulation being determined by the condition that the flow at the trailing edge is parallel to the tangent.

O. Blumenthal, working at the important laboratory at Aix, gave the distribution of pressure arising from this assumption (Z.f.M.u.F., 1913, 31st May, p. 125), and Betz (*ibid.*, 1915, pp. 175-8) has compared calculated with observed

results. Figs. 8 and 9 give the results at 3 degrees and 6 degrees incidence, and the general agreement between theory and wind tunnel measurement is very striking. The observed lift, and, it may be inferred, the true circulation, is about 75 per cent. of the value calculated on Joukowsky's supposition.

E. Trefftz (Z.f.F.u.m., 1913, p. 130) and v. Mises (*ibid.*, 1917, p. 157) develop graphical methods of carrying out the transformation from circle to Joukowsky's profile, of which Miss M. Barker, B.Sc., has given a summary and example (R. and M. 788).

A somewhat different view may be taken of the flow round an aerofoil. No one who has watched the flow of water past a barrier can have failed

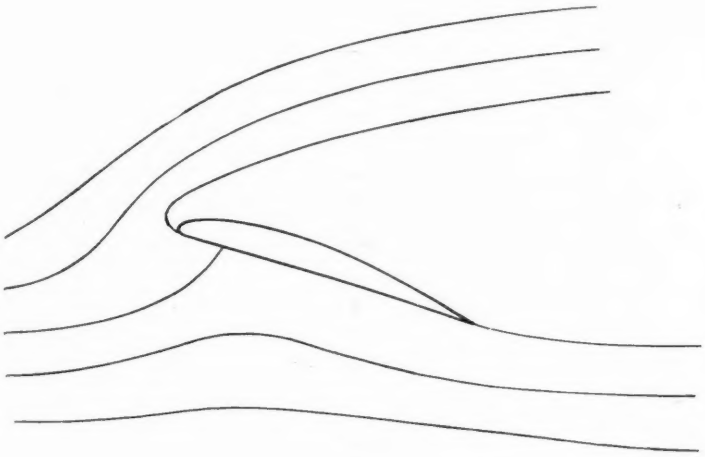


FIG. 13.

to observe the formation of a sheet of separation marked at times by air bubbles drawn in. The motion of the two surfaces past each other forms at once a vortex sheet as nearly as molecular physics will allow (Fig. 13).

The molecular mechanism involved may be easily enough pictured in a general way, but much elucidation by the methods of molecular physics is required before it becomes a subject for computations.

This sheet, as soon as formed, breaks up into a row of finite rotating cores surrounded by fluid in motion very nearly identical with that due to ideal vortex filaments concentrated at the centre as in Helmholtz's theory.

These eddies pass away at first along the line of separation between free stream and dead water, finally becoming confused, mixed together and dissipating their energy in the general body of fluid.

While they continue to exist they maintain a region of approximately dead water behind the barrier, but their breaking down reduces the pressure in the dead water.

The theory of flow with sheets of separation is due to Helmholtz, and Fig. 13 shows the general nature of the streamlines and the shape of the dead water area.

Fig. 14 shows diagrammatically the modification of the flow by viscosity.

At first it would seem that evidence for Helmholtz flow is evidence against Joukowsky's assumption. They may, however, be reconciled *for small angles of incidence*, by the consideration that the layer between the upper sheet of separation and the wing is very shallow and is rapidly set in motion approximating to Joukowsky flow. This is at least roughly confirmed by observation of flow at small incidence. (Cp. Captain Lafay, Technique Aéronautique, 1911, La Photographie du Vent.)

We have still to account for the setting up of circulation. At the face there is viscous friction between wing and adjacent air, proportional to velocity. This sets up a reduction of speed at the lower surface, which is equivalent to the effect of superposing circulation, assuming as before that at a distance the fluid acts as non-viscous (Fig. 14).

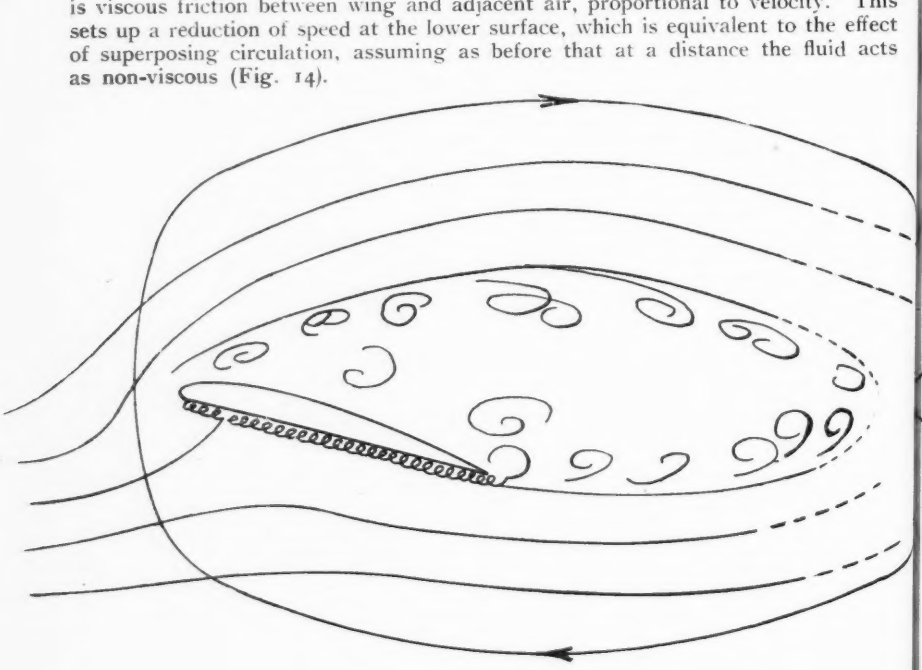


FIG. 14.

On the upper surface, the layer of air between the Helmholtz sheet of separation and the wing, acts as a lubricant and offers less retardation than the direct contact of the stream with the lower surface. But what circulation it does set up is opposed to action of the under surface.

At the same time, the two sheets of separation dissipating in eddies seem to be equivalent to a circulation which is in the same sense as that set up by the under surface.

Comparing the two expressions for the reaction $R=\rho\,\kappa\,U=k_L\,\rho\,c\,U^2$ per unit length, where c is the chord, we get $\kappa=k_L\,c\,U$.

But if we assume that the friction between the surface and the adjacent layers is viscous, the retardation will in fact be proportional to the chord c and the velocity U in agreement with the above relation. Thus k_L is the coefficient of circulation directly and of lift only indirectly.

If now we integrate round a closed path (in the form of a circle if desired for simplicity) large enough to enclose all undissipated eddies we get the approximately ideal circulation of the air at a distance (as an inviscid fluid).

Note added after the meeting:--This is incorrect as a statement of Prandtl's view. The circuit round which the circulation is taken should exclude the vortices which have become definitely detached from the trailing tip of the wing. These are carried down stream with a velocity approaching U, and their influence on the lift is to be neglected.

This very rough descriptive account of the mechanism at least takes qualitative account of the frictional part of the total resistance which separates itself from the induced or pressure resistance of Prandtl's theory, as will be seen below, and is a compromise between the Kirchoff-Rayleigh lift for Helmholtz flow, which is too small, and the Joukowsky lift, which is too large.

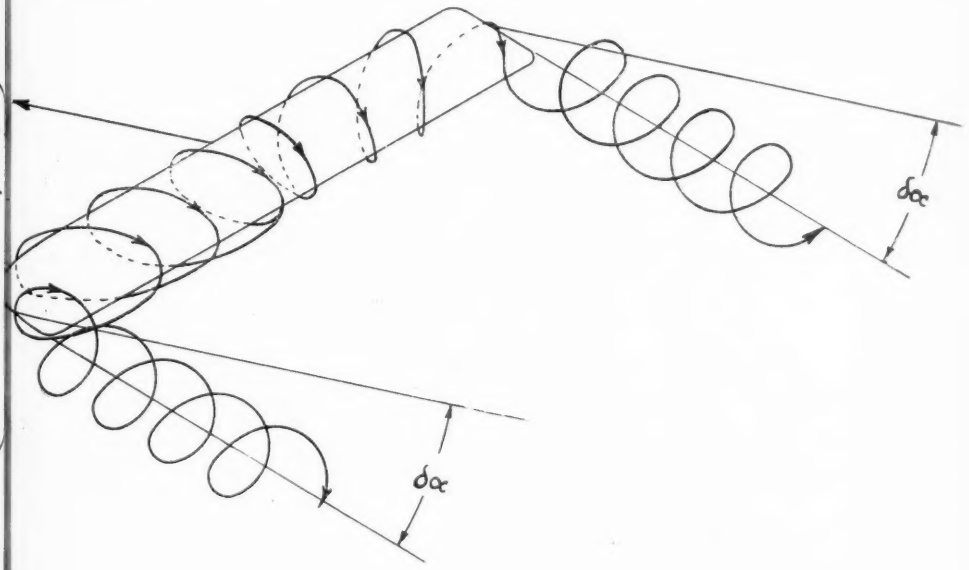


FIG. 15.

It also takes some account of the breakdown of the Joukowsky flow, or the approximation thereto, at small angles, and its transition to the Helmholtz flow modified by viscosity at larger angles. There it must be left at present, for it raises questions in the domain of the kinetic theory of fluids, and of the molecular physics of the formation and break up of eddy sheets, which have not yet been systematically examined, much less solved.

We now pass to consideration of the steps whereby Prandtl extends the Joukowsky method to wings of finite aspect ratio.

Analysing the forces on a wing in the light of these new conditions, Prandtl showed that within the flying range they can be largely accounted for. Circulation having been set up round the wing by the mechanism suggested

above, or by a modification of it, or by any other mechanism, we have vortex filaments distributed along the wing span.

As Lanchester has pointed out, such filaments cannot have their ends "in the air" in a perfect fluid, but must extend to a boundary or to infinity. In air

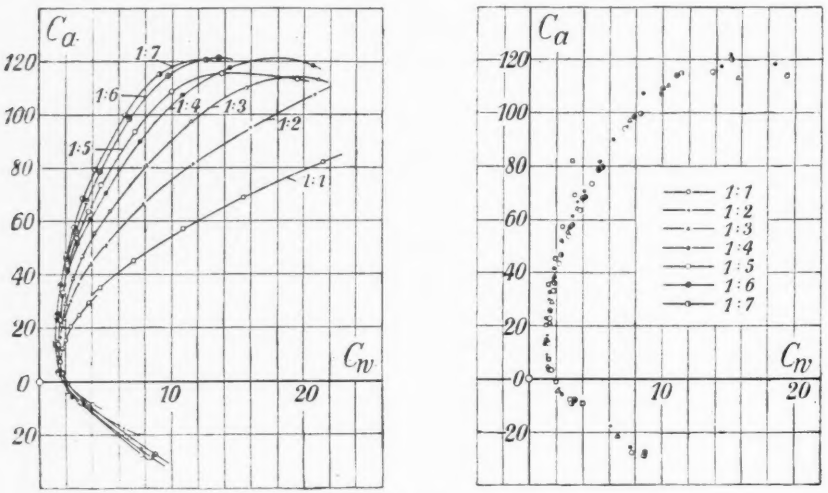


FIG. 16.

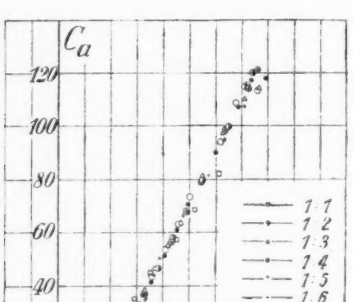


FIG. 16a.

and water the eddy filaments act approximately as in a perfect fluid, and once set up stretch out from the wing. As they leave the wing they are carried backwards by the stream. If the filaments leave the wing tip with a single concentrated core we get the so-called rectangular distribution of lift. But if they leave it in a sheet along the trailing tip we may get the distribution of least

induced drag, the so-called elliptical distribution of lift. For simplicity of discussion the former condition is assumed here. The vortices trailing from the wing tips set up a velocity at every point in surrounding space. In the first place they themselves are carried downwards at a definite angle, each by the other (Fig. 15). They set up at each point along the line of the wing a calculable downward velocity, which alters the relative wind in direction. The total reaction is considered as due partly to the non-viscous forces of reaction, and partly to the viscous forces along the surface. That part due to the non-viscous forces depends on the effective angle of incidence, and is therefore to be corrected for the alteration in the relative direction by the downward component due to the vortices. The other part is due to the surface friction and the tangential velocity near the surface, substantially unaltered at small incidences, by the deflection (Fig. 12), combined with eddy formations similar to those of a symmetrical profile. If then we know the coefficients of lift and drag for a given aspect ratio throughout the ordinary range of flying incidence, we can account for the effect of the downward component created by the trailing vortices, and calculate the lift and drag for any other aspect ratio, on the assumption that the profile resistance is independent of the aspect ratio, and depends only on the incidence and velocity. Examples of such reductions are shown in Fig. 16, and they are very conclusive. (Ergebnisse der Aero. Lab. Göttingen, I., 1921.)

Professor Bairstow (Applied Aerodynamics, 1920, p. 364) objects that Kutta's theory cannot indicate even the possibility of the well-known critical angle of an aeroplane wing, while the best he has to say for it is that the disagreement between calculated and observed values is not so great as to discredit the theory. The criticism is true of all theories of moving fluids which are assumed to be physically homogeneous. It is true even of Professor Bairstow's recent research work. It requires the methods of molecular physics to attack the problems of the molecular mechanism of moving liquids, of which the kinetic theory of gases gives glimpses.

But the theory of Lanchester and Prandtl finds an application and an independent verification in airscrew theory. If the blades act substantially as aerofoils, then circulation will be set up in exactly the same way, and trailing eddies will be generated at blade tip and root. The eddies at the root will be of the same sign, and will amalgamate into a single eddy with a single air core along the axis. The eddies at the tips will also be all of the same sign, opposite to that of the axial eddy, and their cores will, in the first place, leave the tips in the direction of the blade path through undisturbed air, thus forming a spiral core. Each spiral will impose a velocity on every point of the surrounding fluid, and the self and mutual influence of the spirals will determine their final steady position. The steady position and strength being known, the influence of each eddy may be calculated.

Lanchester (Aerodynamics, § 217, 1911 Edition) states that this phenomenon was observed by him from the after-deck of the s.s. "New York."

A glance at Flamm's photographic record of the formation of such vortices with air-filled cores, trailing spirally from the tips of a model screw blade, will bring conviction to the most cautious that the theory has an indisputable experimental basis. ("Die Schiffschraube, Berlin, 1909.) With stereographic apparatus the spiral cores of the trailing vortices, filled with air, can be seen vividly in three dimensions. The four spirals shown appear as left-handed. In Plate 10 (Fig. 3) and Plate 9 (Fig. 4) two vortex cores are seen clearly originating at different points on the same blade tip. They interlace and finally amalgamate. In Plate 9 (Fig. 3) a single core originates at each blade tip. The pitch remains very uniform for several turns, but once they begin to lose this form, complete irregularity soon follows, so that the steady position seems to be unstable.

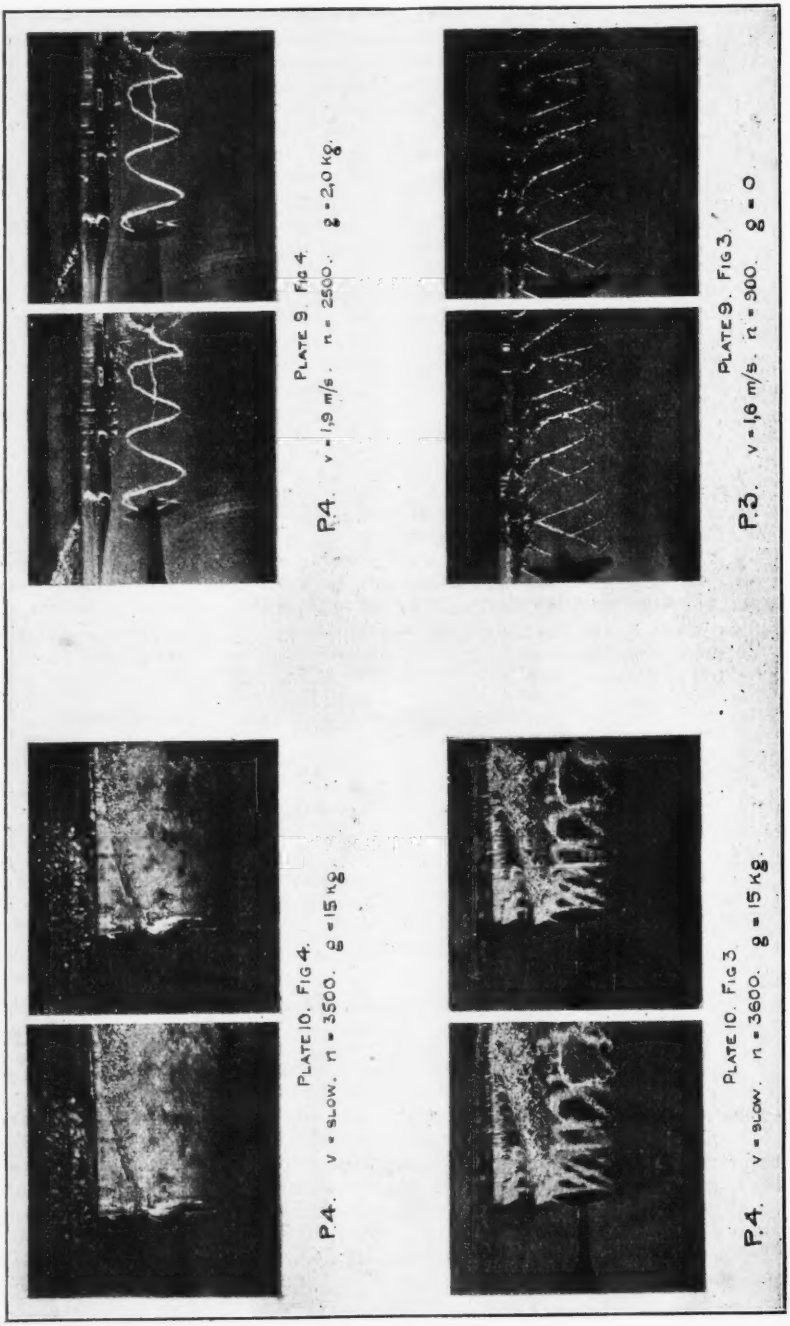


PLATE 9. FIG 4.

P.4. v = 1,9 m/s. n = 2500. g = 2,0 kg.

PLATE 9. FIG 3.

P.3. v = 1,6 m/s. n = 900. g = 0

PLATE 10. FIG 4.

P.4. v = SLOW. n = 3500. g = 1,5 kg.

PLATE 10. FIG 3.

P.4. v = SLOW. n = 3600. g = 1,5 kg.

Adopting the hypothesis of a single core at each tip, if we know the pitch and circulation we can calculate the effect at any point in space (Fig. 17). The formulæ are given in the appendix. The speaker has not been able to find them in the literature of this subject or of the related subject of the magnetic field set up by currents in open spirals. He has made a good deal of progress in computing and tabulating the integrals, but heavy translation work has prevented their completion for this paper.

Fig. 18, which is similar to the figure in Prandtl's original paper (Abriss der Lehre v.d. Flüssigkeits Bewegung, Handwoerterbuch der Naturwissenschaften, Vol. IV., 1913), gives the components of velocity in the plane of the paper. A peripheral component also exists in the actual spiral stream lines.

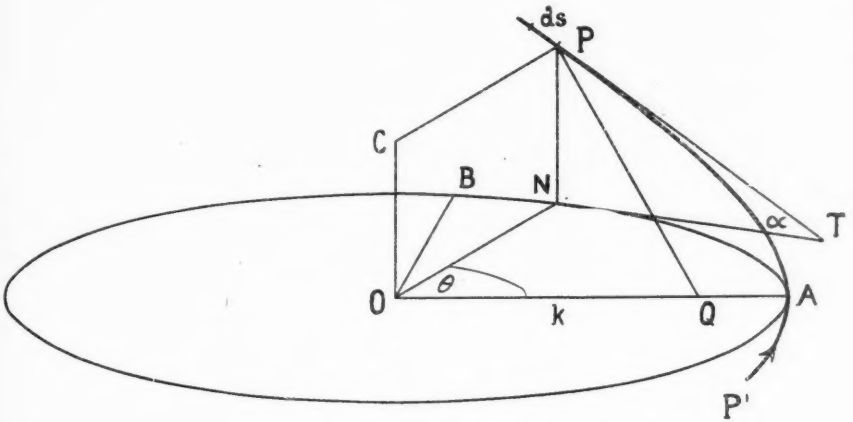


FIG. 17.

We may now find at each element of the blade the relative wind velocity corrected for the effect of the spiral and central vortices, and using the coefficients of lift and drag for infinite aspect ratio, compute the reaction on each element.

Such is the Lanchester-Prandtl theory of water and air screws, in the barest outline indeed, but, it is hoped, without the omission of any known physical phenomenon, so far elucidated.

Mr. Glauert gives an account of the theory in R. and M., 786 (which came into the speaker's hands a month ago—October 23). By taking the vorticity as distributed uniformly in a spiral belt covering completely the cylindrical surface of the outflow, the problem is reduced to one whose solution is known in electromagnetic theory. But in this way he loses one of the most characteristic effects, that of the high velocity near the blade. With a large number of narrow blades his assumption might be useful, but with a small number of blades, super-position

of the velocity due to the vortex and of the circulation round the blade on the undisturbed flow will give just such pulsations of velocity as were observed by Drzewiecki.

Again the portion of the vortex near the tip has the greater effect on the part of the blade which does most of the work. The other vortices have less effect in the order of the inverse of the distance of the blade tip from which they spring.

Thus de Bothezat's conclusion is negatived and Drzewiecki's original assumption is very largely justified, and the cascade arrangement is seen to be a rational approximation.

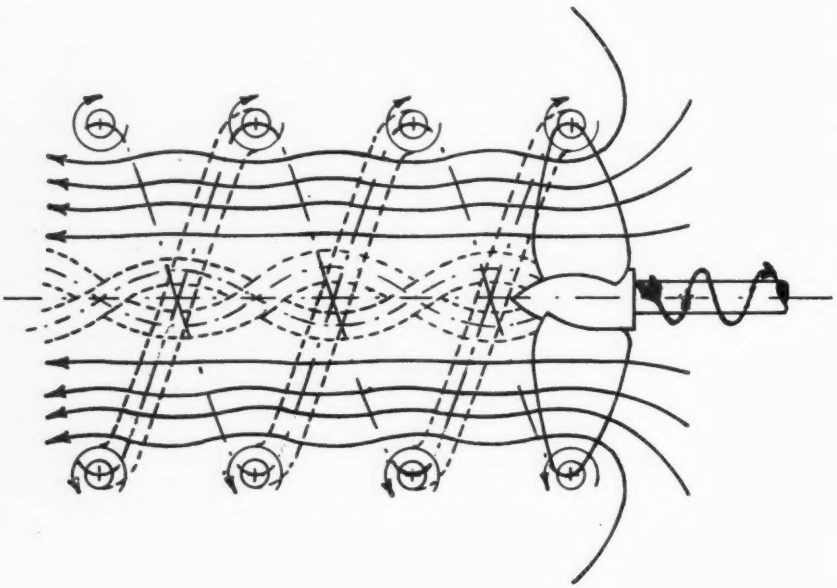


FIG. 18.

Self-interference is the most important factor in so-called inflow correction, and it is largely accounted for in the lift and drag coefficients of an aerofoil of the same profile and the same order of aspect ratio. The reduction of the inflow correction for blade incidence is almost complete, in so far as it differs from the aspect ratio correction.

Since this Paper was undertaken, M. Margoulis has published a lengthy paper on helicopters (Aérophile, Supplement), and Mr. Case is contributing articles to this journal. At the same time the development of Prandtl's theory has increased the present lecture to such length that it has become necessary to abandon any lengthy examination of helicopter theory.

APPENDIX I

Let P′AP (Fig. 17) be an arc of a helical vortex core; θ the angle between generators OA, CP; α the angle between any tangent PT and the plane AOB. Q is any point on the axis of a blade, cutting off a fraction k of its length, taken as unity for simplicity.

To find the components of velocity at Q due to the element ds of strength -1 (left-handed).

Projecting unit length of TP on the axes OA, OB, OC, the components are seen to be

$$-\cos\alpha\sin\theta,\ \cos\alpha\cos\theta,\ \sin\alpha.$$

The point Q is $(k,\ o,\ o)$.

The point P is $(\cos\theta,\ \sin\theta,\ \theta\tan\alpha)$.

The components of unit length along PQ are

$$(k-\cos\theta)/r,\ -\sin\theta/r,\ -\theta\tan\alpha/r,$$

$$\text{where } r^2 = (k-\cos\theta)^2 + \sin^2\theta + \theta^2\tan^2\alpha.$$

The vector product of unit vector along ds in negative sense, and of unit vector along PQ has components given by dropping the first, second and third columns of the matrix.

$$(-\cos\alpha/r)\ \begin{vmatrix} -\sin\theta & \cos\theta & \tan\alpha \\ k-\cos\theta & -\sin\theta & -\theta\tan\alpha \end{vmatrix}$$

These must be multiplied scalarly (numerically) by $ds/4\pi r^2$ to get the actual velocity components, hence putting $ds = d\theta/\cos\alpha$ and integrating

$$V_x = -\tan\alpha\int_o^{\cdot}(\sin\theta-\theta\cos\theta)\,d\theta/4\pi r^3$$

$$V_y = -\tan\alpha\int_o^{\cdot}(k-\cos\theta-\theta\sin\theta)\,d\theta/4\pi r^3$$

$$V_z = -\int_o^{\cdot}(1-k\cos\theta)\,d\theta/4\pi r^3$$

For any point Q $(k\cos\gamma,\ k\sin\gamma,\ o)$ in the plane of a generator at angle β with a blade axis, these expressions are easily generalised and become

$$V_x = -\tan\alpha\int_{-\beta}^{\cdot}(\sin\theta-k\sin\gamma-\theta\cos\theta)\,d\theta/4\pi r^3$$

$$V_y = -\tan\alpha\int_{-\beta}^{\cdot}(k\cos\gamma-\cos\theta-\theta\sin\theta)/4\pi r^3$$

$$V_z = -\int_{-\beta}^{\cdot}(1-k\cos[\theta-\gamma])/4\pi r^3$$

$$r^2 = 1 + k^2 - 2k\cos[\theta-\gamma] + \theta^2\tan^2\alpha.$$

The upper limit is somewhat indeterminate, but the integral converges rapidly after the first few spiral turns.

Discussion

The Chairman said they had listened to an extremely interesting account by Major Low. Perhaps it was not so contentious as he had indicated at the beginning. Nevertheless, there was a good deal of material for discussion in the paper and there were several people present who were able to speak with knowledge on the subject.

Mr. McKinnon Wood said he did not know whether the lecturer had had the intention of giving an historical account of airscrew theory, but thought he was rather giving a logical account in placing the multiplane interference before the inflow theory. Historically, the Drzewiecki theory was followed by the application of inflow factors before, he believed, the multiplane idea was put forward, at any rate before the multiplane theory was developed. A great deal of credit must be given to Drzewiecki because his theory had formed the basis of practical airscrew design, and on that designers had worked with great success, although it suffered from the weak point theoretically that it did not define very clearly what an aerofoil element was. It said that the airscrew might be regarded as built up of aerofoil elements, but until we came to the work of Prandtl we never had a very clear definition of an aerofoil element. The actual airscrew designer simply took the data which were supplied him from wind channels which was obtained on aerofoils of aspect ratio 6, and fortunately that worked very well for some time, while the power transmitted was relatively small. At the same time, it was realised that the Drzewiecki theory might lead to calculations of efficiency which were so high as to be inconsistent with the Froude-Rankine theory. Various people had tried to work in the Froude-Rankine theory with the Drzewiecki theory, but they mostly realised that what they were doing was empirical to a large extent, and they therefore introduced empirical instead of theoretical factors and that had worked fairly well. The multiplane idea, the use of an "infinite" series of aerofoils as a means of determining the interference between the blades was an endeavour to replace the empiricism by a rational basis. It was a logical development of the Drzewiecki theory, but again, in his opinion, it suffered from the same lack of a clear conception of the "aerofoil element." If we experiment with aerofoils of aspect ratio 6 we get one answer for the interference, but if we use another aspect ratio a different answer would be obtained. The Drzewiecki theory was sound as a basis, but the interference of the aerofoil element had to be introduced into the problem. We should start with an aerofoil of infinite aspect ratio and apply corrections for interference between the blades and also for the interference which corresponds rather loosely to the aspect ratio correction for the ordinary aerofoil. At the end of the paper Major Low said:—"Self-interference is thus the most important factor in so-called inflow correction and it is largely accounted for in the lift and drag coefficients of an aerofoil of the same profile." He could hardly follow Major Low in this. He did not understand what was meant by self-interference. If by that was meant the disturbance of the air flow due to the aerofoil element itself, that did not seem to him to be interference. It was simply the flow that was present.

Major Low said it might meet the point if he used the words "with respect to infinite aspect ratio."

Mr. Wood further quoted:—"The demolition of the inflow correction or blade incidence is almost complete, in so far as it differs from aspect ratio correction." The term "aspect ratio" applied to an airscrew had always given difficulty. The aspect ratio of an aerofoil had a perfectly definite meaning, but there had always been a difficulty in assigning any meaning to "aspect ratio" for an airscrew blade.

There was a reference in the paper to Mr. Glauert's work recorded in R. and

M. 786. He did not think Major Low had given quite a fair account of it. This was based upon the work of Prandtl; but he knew of no success that had been achieved in developing an airscrew theory in Germany. It was a curious application of the Prandtl theory, because it seemed to eliminate the vortex arriving again at the two inflow factors of one half the slipstream speeds, but with the aerofoil element clearly defined as part of an aerofoil of infinite aspect ratio.

Early in the paper there was a reference to the R.A.E. experiments on flow about an airscrew whose tip speed exceeded that of sound and it was added that in this state the axial columnar outflow disappears and radial outflow is set up. In that particular experiment that is what actually happened, but he wished to make it clear that that was not necessarily due to the tip speed exceeding the speed of sound. More recent experiments had shown that the flow through an airscrew might be of the normal type when the tip speed exceeded the velocity of sound by 20 to 25 per cent. The experiment to which the lecturer referred was rather a freak experiment. The airscrew was run at a fixed point by an electric motor and was an airscrew of very low pitch with very fat sections.

Mr. R. McKinnon Wood (*communicated*): I do not follow Major Low's conclusion that the inflow correction is "almost completely demolished." May I ask space to make my difficulty clearer?

The use of the theoretical inflow factors of Froude and de Bothezat in a calculation using aspect ratio 6 aerofoil data is irrational; but Glauert's analysis based upon the Prandtl vortex theory leads to the use of just these factors with infinite aspect ratio data.

If by "self-interference" Major Low means the "induced velocity" due to the vortex system trailing from the blade under consideration, I agree that it constitutes the greater part of the interference in a two-bladed screw; but it can, I believe, be shown that in practical cases the interference is roughly proportional to the number of blades and self-interference not so very much greater than that of any one other blade and not the greater part of the whole in a multi-bladed screw.

Further, interference in an airscrew differs from the induced velocity due to the trailing vortex system of an aerofoil (aspect ratio correction), because the vortices are coiled behind the screw in fairly close helices and the induced velocity is therefore greater.

This helical form of the vortices is a consequence of the helical path of the blade and but another aspect of the idea that the blades chase their own tails by virtue of rotation in addition to the forward motion—the idea represented by the development in an infinite cascade. The vortex theory thus confirms the existence of mutual interference between blades (in which we may include the influence of the blade on itself due to its helical path).

It is interesting to note that Glauert has reached the conclusion that the performance of any anular element of the airscrew disc is independent of the performance of the other anuli, which somewhat weakens resemblance between interference in an airscrew and aspect ratio effects on an aerofoil.

Mr. Kirdany said he was not very clear on one point. The lecturer showed an aerofoil with a pointed tail and the flow coming out tangentially to the upper and lower surfaces. He happened to be working on this problem and this tail gave him a lot of trouble and it was for this reason that he would like to know exactly what happened there. Is the pointed tail essential to the mathematical transformation or is the shape of tail immaterial? Also what would happen if the cyclicity put on was more than that necessary to drive the backward stagnation point to the tail? Would the latter go underneath the wing or what?

Mr. A. FAGE said that one could not help being impressed by the extensive reading necessary for the preparation of a paper such as this and he would like to congratulate Major Low on his labours. He agreed that the development of airscrew theory on the lines suggested by Prandtl and Lanchester appeared to offer very promising results, especially if the realms of empiricism were left behind. Perhaps he would be excused if he confined his remarks to the work done at the N.P.L. as that was the work with which he was most familiar. On page 2 of the paper it was stated:—"Accordingly it has been proposed to take half the total increase in mean velocity as altering the true angle of incidence of the blade elements." Unless he was mistaken, he took it that the lecturer was there referring to an empirical theory which was a combination of momentum and aerofoil theories and which was published several years ago. That was based on a quasi-rational basis but had now definitely given way to the later theories mentioned by Major Low. The shortcomings of this theory were also demonstrated quite clearly by the latest work at the N.P.L., which was published a few months ago. This research endeavoured to estimate from comparisons of the pressure distribution over an airscrew blade and over aerofoils of appropriate shape the aerodynamic performance of an element as actually functioning in the blade, and also the type of flow which must be assumed to exist if these aerodynamic data were used in the calculation of the performance of the airscrew. The research showed that the performance of the blade element was in most cases appreciably different from that measured on the rectangular aerofoil of aspect ratio 6; the magnitudes of the translational and rotational inflow velocities which should be used with the aerodynamic data of the blade elements were also estimated. The last figure of that report showed that the translational inflow velocity which should be used with the aerodynamic data of the blade elements was different from that calculated by the Froude theory. He hoped that now that a lengthy investigation on a family of airscrews had been brought to a conclusion the opportunity would be taken to examine these results in the light of the theoretical work of Prandtl and Glauert. He would like to support the remarks of Mr. Wood that Mr. Glauert's theory was a brilliant piece of work and appeared to be a logical extension of the earlier work of Froude.

The CHAIRMAN said he would speak mainly as a critic of the Prandtl theory. First of all, he wished to say with the lecturer, that he was greatly impressed by the effect of the Prandtl theory in bringing experiments on aerofoils of a given aspect ratio into agreement. He was also impressed by the fact that on one of the slides shown by the lecturer the comparison between the calculations and the experiments of Betz on pressure distribution agreed to a degree not hitherto known. There was some little difficulty in that case, however, because the calculation was made on a Joukowsky aerofoil and it was essential to the success of the method that the aerofoil should have a sharp cusp, otherwise it would be found that instead of having a very small pressure at the trailing edge of the wing the value would again come out to $\rho v^2/2$. He did not think it mattered very much, however, because probably the area of intense pressure would be very small and would not affect the resistance very much, but so far as he could see, it would have to be there. The Prandtl theory did help to connect a great number of facts and, to his mind, was a very good empirical theory. But he would ask them to be chary of scrapping all their previous work and placing sole reliance on Prandtl's theory, because in his view it was not sufficiently well established. There was nothing wrong with the old idea of the Froude and Rankine theory or the Drzewiecki theory, except their degree of approximation. Whether the Prandtl theory went one stage further or not was a matter to be discussed, and it appeared that it had a chance of doing so; on the other hand, the physical fact, stated as a consequence of Newton's laws, that thrust is equal to momentum produced per second still remained and, before dismissing the inflow theory, one must get to know a good deal more about the Prandtl theory. It was rather surprising to

him to find that the lecturer had got through the whole of his lecture without mentioning a fundamental property of air on which its motion depends, viz., its viscosity, and that was not peculiar to the lecturer; it was the defect of the "circulation" theories of lift. In the Prandtl theory viscosity had no other place than to produce circulation and one of the slides shown by the lecturer referred to a cylinder; the spin on it made the illustration a little more real, but in plain language the Prandtl theory would mean that circulation round the cylinder produced lift without drag. That brought him to ask the questions: When should a circulation be applied and when should it not; how much should be applied and so on? The Joukowsky aerofoil had to have a pointed sharp tail in order to give a precise example since circulation applied to that aerofoil did not give infinite velocity at the trailing edge. On a cylinder any circulation would do, and if the trailing edge of a wing section were rounded, again there was indeterminateness. Major Low had called attention to certain remarks in his (the Chairman's) book and which he repeated in his lectures from time to time. He had pointed out in connection with the Lanchester theory of cyclic flow that none of these theories could predict stalling, which was one of the most important things in aeronautics. A Joukowsky aerofoil 90 degrees to the stream would not stall. The lecturer then says, "The criticism—that you cannot predict stalling—is true of all theories of moving fluids which are assumed to be physically homogeneous." He (the Chairman) wished to give that the direct denial. It just was not true. The great service of Stokes in relation to the problem of solving the equations of fluid motion was to remove it from the theories of molecular structure. Stokes knew that fluids were made up of molecules and that the movement of these molecules gave rise to viscosity. But this did not alter the fact that although they did not know what the molecules were doing, yet their effect, so far as resistance was concerned, was expressed by a coefficient of viscosity, and if they assumed that the material was homogeneous and viscous they could, from the equations of motions, deduce stalling. Of that he was convinced from experimental evidence. This brought him to his fundamental objection to the Prandtl theory. They could have various theories which were good or defective in various proportions, but ultimately if they were going to deal with a real physical problem they must come back as the basis to physical ideas. They had in the equations given by Stokes, and the txperiments of Poiseulle and Stanton, very strong experimental indication that these equations were sufficient to account for the phenomena, whether it was a steady flow or an eddying flow. These equations did not appear in the Prandtl theory. Other equations were given, but so far as he knew the literature of the subject nobody had attempted to show what relation these fundamental equations had to the viscous equations. It happened that at the present moment various people at the Imperial College were working on the solution of equations of viscous fluid motion and they naturally looked for the source of the circulation of which the Prandtl theory makes use, without finding it. In the solution of Stokes' equations it appeared there was no circulation, *i.e.*, the motion of a viscous fluid around a body moving in it was free from circulation. He knew of no natural mechanism that could produce circulation in a viscous fluid and that seemed to him to make a great difference to one's appreciation of the Prandtl theory. It was not, in the sense of Stokes' equation, a fundamental theory, and the only justification which the lecturer had given was that the results fit experiment, *i.e.*, it was one degree of further empiricism. Moreover, Prandtl himself, it seemed to him, did not claim nearly as much for his theory as other people claimed for him. Prandtl himself realised, and he believed also that Lanchester realised, the very tentative nature of the proposals and he did not think any of them should run away with the idea that the Prandtl theory was the last word in either aerofoil theory or airscrew theory. We were just beginning to find out where it might fit, but the ultimate solution must be along other lines. We must start from the Stokes' equations of viscous fluid

motion and if the Prandtl theory was going to help it must show how its hypothesis of circulation had its equivalent in what we know of the real equations of viscous fluid motion. He would again like to say that he did not wish to be a harsh critic of the Prandtl theory; he only wished to come in at a meeting of this description and to say that the case is not yet overwhelmingly proved.

Mr. ZAHRA said he had a difficulty in explaining the cyclic theory physically. The mathematical explanation given by the lecturer appeared to be to subtract from the actual physical fluid motion the cyclic factor and that that subtraction would give the motion of an imperfect fluid, and that when that subtraction was added again to the imperfect fluid flowing past the cylinder, one obtained the flow of motion which would give the lift. As a matter of fact, when he added that subtracted factor into the imperfect fluid motion he did not get the physical flow at all, because from what had been said by Prof. Bairstow in one of his lectures, although the cylinder was put symmetrically and the flow past it originally was along the central line, the stagnation point started from the lower part and there was no similarity of flow between top and bottom of cylinder although gravity was not taken into account, and this showed that the theory when applied to a cylinder would not give the exact line or lines of flow which ought to appear experimentally. The other point he wished to mention was the 25 per cent. mistake between the experiment and the calculated value. It appeared that this always existed in spite of the fact that the strength of the cyclic current was so chosen as to fit the experiment.

Mr. LOCK congratulated the lecturer on the excellent photographs he had shown of helical vortices coming off the blades of a screw under water. That was a very striking confirmation of the application of the Prandtl theory to the airscrew, and the lecturer had stated at the end of his paper that it helped to demolish the inflow correction. But Glauert in his application of Prandtl's theory to an airscrew had shown that the helical vortices coming off from the blades gave rise to an inflow and confirmed the inflow factor $\frac{1}{2}$ for aerofoil data reduced to infinite aspect ratio. It also confirmed the standard inflow theory except that the latter made use of data from aerofoils of aspect ratio 6 and so required a different inflow correction. Of course, Glauert's theory would have to be further confirmed by experiment, but it seemed that in any case it was a convenient theory for actual computation in connection with airscrews. The lecturer mentioned that Prof. Greenhill had produced a theory in which the rotational slipstream produced a thrust, and he would like to know how that was possible; how it could be reconciled with the idea that the thrust is equivalent to momentum in the slipstream.

Major LOW said he would prefer to take time in replying to the discussion, but there were one or two points he could answer now. One was about the aspect ratio in an airscrew. He did not define it in the lecture and he had tried to make that clear in his comments during the reading of the paper. He had indicated calculations by means of Prandtl vortices given the strength of the vortex for calculating the real incidence of each blade element. The fundamental aspect ratio was infinite, as was the case all through the Prandtl theory; all the corrections were based on the infinite aspect ratio. With regard to the multiplane theory, he thought it was a perfectly sound and logical development of the Drzewiecki method, but it would have to be regarded finally in the light of the Prandtl theory. With regard to the experiments on high speed blades and the tip speed exceeding the speed of sound, he had there quoted a definite and interesting experiment and accepted the comment. With regard to Mr. Kirdany's question, the cusp at the tail of the wing profile was a mathematical condition in Joukowsky's theory. He did not suggest that it did anything more than give an approximation to the actual state of flow, and in the first place his position

was based, as the Chairman very justly remarked, on the agreement between the measurements and the experiments. With regard to the Chairman's remarks on the physical theory of fluids, he noticed that Mr. Lanchester, in one of his papers, had said that if we had waited for the mathematicians we would not have made any progress since the days of Noah, a somewhat one-sided statement. He would be glad to see adequate endowments provided so that pure scientists might have the leisure and atmosphere for pursuing purely and strictly abstract studies, for such studies were the real foundation of technical developments. But as an engineer he did not intend to wait for them on this occasion. He believed that that attitude really covered all the admirable remarks of the Chairman on the very difficult subject of hydrodynamical theory. He himself was not a physicist and it would be affectation on his part to argue with the Chairman on his special branch of physics. At the same time, he thought he had followed clearly enough the outline of the question of viscosity. Prandtl's assumption that viscosity sets up circulation, and that once it is set up the fluid could be regarded as acting as a perfect fluid, had the support of no less an authority than Mr. G. I. Taylor, and he might put that against the Chairman's statement. With regard to Mr. Glauert's theory, he had brought his paper rather rapidly to a close and did not make it quite clear that the Prandtl theory squared with the Drzewiecki stroboscopic experiment; that near the blades and near the cores of the vortices which were seen coming off from the tips of the blade there were very high local velocities. The effect of a vortex coming off one blade upon the other blade was relatively quite small. This agreed very substantially with Drzewiecki's actual stroboscopic experiments. On the other hand, Mr. Glauert assumed a very large number of blades and thus used a type of Froude actuator; only instead of the flow being axial it was actuated spirally through the blade disc; he thought this was a possible point of view, to go back to Froude and consider the mean increase of velocity. And by exactly the same reasoning as Froude's, they got half the final spiral velocity at the actuator disc,* and rotational inflow velocity would also come in as a correction. Taylor (R. and M. 765) had also pointed out that in Pannel and Jones' observations (R. and M. 371) there was no trace of the rotational inflow in front of the blade, and that conclusion again squared quantitatively with this mechanism of the central axial vortex and the spiral vortex coming from the blade tips and the circulation round the blades. If they estimated the rotational velocities in front of the blade, due to the circulation and vortex system, they would find it was very small. He was quite in agreement that Glauert's version of Prandtl's theory should be called Glauert's theory if the author really so desired. He himself thought it was merely a degenerate† form of Prandtl's theory, but if Mr. Glauert thought otherwise he was perfectly entitled to argue his view. As to the question with regard to Prof. Greenhill, he must refer them to Prof. Greenhill's own argument ("The Dynamics of Mechanical Flight," 1912, p. 102). He would reply in writing to such points as had not been dealt with.

On the motion of the CHAIRMAN a hearty vote of thanks was accorded Major Low at the conclusion of the discussion.

Major Low (*contributed*): With regard to Mr. Lanchester's letter to the Secretary putting forward his later work as his most complete statement, I have read several times his two papers on the Aerofoil and Airscrew, and agree that they are a further development of his original theories, but as they are subsequent to much more systematic work in Germany, they can scarcely claim the same importance as his earlier work, which occupies a unique position in the development of what I have called the Lanchester-Prandtl theory.

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* NOTE.—It is necessary to distinguish between the expressions before, at, and behind the disc.

† NOTE.—In the sense that the trigonometrical functions are degenerate cases of elliptical functions.

Mr. Riach also has communicated his desire that his recent paper in the Journal should be considered as most representative of his views. He has there made full use of the ideas suggested by Drzewiecki's stroboscopic experiment, and as already stated above, this along with the "cascade" experiment may offer a useful check. But while logical and rational, I think it cannot be regarded as comparable with the circulation theory as a physical statement of what is actually taking place round the blade.

Fig. 14a is added as a better representation of flow at small incidence.

FIG. 14a

The Helmholtz sheet at the upper surface is prevented, partially at least, from breaking up by its image in the surface. It curls in on itself, thus approximating visibly to the assumed Joukowsky flow. At higher incidence it reverts to the Helmholtz flow, modified as in Fig. 14.

Since the meeting the writer has received a paper by Betz with a note by Prandtl, on the application of the vortex theory of wing lift to airscrews.*

This is referred to by Mr. Glauert, R. and M. 786, App. IV., and contains a discussion of the simplified form of the theory ascribed to him. Assuming, as in this simplified form, that there is an indefinitely large number of blades of indefinitely small section, the airscrew is reduced to Froude's ideal actuator disc as far as axial components of flow are concerned. Prandtl's remark that an inflow correction of one half the final increase in velocity must be applied at the disc is in accordance with the assumptions made. It is of course implied, if not explicitly stated, that the uncorrected coefficients are those of profiles with infinite aspect ratio. With regard to the rotary component of flow Prandtl makes the analogous assumption that at the disc a correction must be made of half the final rotary velocity. The disc is a surface of discontinuity of pressures for axial flow, but of velocities for rotary flow. It is in fact a vortex sheet with vortex filaments directed radially, and there is no rotary flow before the surface, while behind the full rotary velocity is at once set up.

To give the expression "at the disc" a physical meaning, the disc may be supposed to have a small finite thickness, and the vorticity not to vary in passing through the disc at right angles to the surface. In accordance with the electromagnetic analogy, the rotary component at mid-thickness will have one half the final value, and this is the meaning to be given to Prandtl's rule. This rotation is in the same sense as that of the screw, therefore it is to be taken as subtractive.

* Nachrichten v. d. K. Gesell. d. Wissenchaften zu Goettingen, Math-Phys. Kl., 1919, Heft 2, p. 193, "Schraubenoropeller mit geringsten Energieverluste."

In the more practically important case of airscrews of two, three or four blades, the writer's exposition of the consequences of applying the wing theory to airscrews is confirmed. The integrals in the appendix and the Fig. 17 of this paper are identical, *mutatis mutandis*, with integrals 9, 10 and 11 and Fig. 4 of Betz's paper.

It is at once disappointing to find one's work anticipated, and satisfactory to find it in agreement with the work of a recognised authority.

The contributed remarks of Mr. McKinnon Wood, Mr. Glauert and Mr. Lanchester will require careful consideration before reply. But I must refer Mr. Lanchester to p. 102 of Sir George Greenhill's "Dynamics of Mechanical Flight" for the very special condition under which he obtains the result questioned. Perhaps too much importance is given to this case, but it has been quoted by an American writer and this led to its inclusion in the references.

Mr. H. GLAUERT (*contributed*): I should like to emphasise one aspect of airscrew theory, namely, the distinction which must be made between the actual flow through the disc of the airscrew and the interference flow experienced by the blade elements. This distinction was pointed out in the multiplane interference theory and is also an essential feature of my more recent theory based on Prandtl's vortex theory of aerofoils. The actual disturbance consists of two parts, the first being due to the circulation round the blade element under consideration and the second due to the remainder of the airscrew. Only the second part acts as an interference on the blade element under consideration.

This view of the nature of the interference experienced by the blade elements leads me to differ from Major Low on certain points. I agree, of course, that the axial flow through the disc of the airscrew is strongly periodic, as shown by the stroboscopic measurements. On the other hand, it appears to me that this periodicity is due mainly to the circulation round the blade elements, and this part of the disturbance must be subtracted from the actual flow to obtain a measure of the interference flow experienced by the blade elements. The axial interference flow is the velocity field of the trailing vortices which constitute the slipstream, and since a helical sheet of vortices springs from each blade of the airscrew it is clear that the interference flow will also have a periodic character. This periodicity will, however, be less strongly marked than that of the actual flow shown by the stroboscopic experiments, and I am satisfied that it is sufficiently accurate to take a mean value in estimating the interference effect. By this method the old "inflow" factor $\frac{1}{2}$ is restored to its position and, when used in conjunction with aerofoil data for infinite aspect ratio, leads to very satisfactory agreement between calculated and observed airscrew characteristics.

Mr. F. W. LANCHESTER, M.Inst.C.E. (*contributed*): Major Low in his paper has made very generous reference to my own work in connection with the screw propeller and has given me an opening for adding a few remarks to what he has himself said. There are two points of view from which to regard the theory of any mechanical device. The one is to consider the theoretical work as an aid to the practical man in the development and perfection of his mechanism; the other is to treat the question as the more academic problem of solving a mathematical or quasi-mathematical puzzle. My own efforts have been directed to the former rather than to the latter. I note that Major Low complains of the absence of any account in my work as to the manner in which I arrived at my conclusions, and also of the absence of a reference to the work of others "from whom he may be supposed to have drawn suggestions or experimental evidence." With regard to the first of these I do not think he can have read my "Aerodynamics" seriously or he would realise that what I did in that work was a perfectly logical investigation which led step by step to formulated conclusions. While it is perfectly true that my own work lacked the precision of a real mathematical or Euclidical demonstration, there were no steps in the argument omitted and no results given which were not supported by the argument and evidence presented. There are some

39 pages constituting this investigation forming the first part of Chapter IX. of my "Aerodynamics" devoted entirely to propulsion and the theory and design of the screw propeller. The work of Rankine and Froude is discussed at some length under the title of the Newtonian Method. Then there are some six pages constituting a close investigation on the question of efficiency. Following this there is a discussion concerning the proper distribution of the load radially on the propeller disc area and of the number of blades permissible, and on interference, leading to some suggestive work on the subject of actual propeller design and the application to a hypothetical case of an aeronautical propeller. I think I am in a position to refute any statement to the effect that there is an absence of any account as to the "manner in which Lanchester arrived at his conclusions." Beyond the above Major Low omits even to mention two later papers which I have published on propulsion and the screw propeller, namely, "A Contribution to the Theory of Propulsion and the Screw Propeller," read before the Institution of Naval Architects in 1915, and a paper read a few weeks later on "The Screw Propeller," before the Institution of Automobile Engineers. The latter required to be read in conjunction with a paper read a month previously before the same Institution on "The Aerofoil." I will not attempt to abstract either of these papers here, but without undue egoism I do not think it can be pleaded that a review of airscrew theory is complete without some mention of this work.

When Major Low complains that I have not made due reference to the work of others, I must refer him to the very extensive reference to Rankine and Froude above cited. At the same time I confess ignorance of the work of Drzewiecki. In self-defence I would point out that, as stated in my "Aerodynamics," a large amount of the investigation then published (1907) actually dated from ten or twelve years earlier; a great deal of the subject matter of my investigation *re* the aerofoil and reference to its application to the screw propeller were included in a paper which I offered to the Physical Society of London in 1897. The fact that this paper was not accepted was not my fault—I did my best and a great deal of the matter in this paper has since (as Major Low says) received its principal recognition in Germany. Perhaps I ought to have sent the paper in question to a German scientific society, but I was a young man at the time and did not realise then how true it is that an Englishman has usually to go for recognition to some other country than his own. With regard to Mr. Drzewiecki's work, when I first heard of this, several years after the publication of my "Aerodynamics," I tried from more than one bookseller, also by writing direct to various addresses abroad, to obtain a copy or some particulars of the work in question and I failed. The only information I have now in my possession was furnished me by the Air Ministry three or four years ago.

As far as I can make out there is a decided difference between Mr. Drzewiecki's ideas and work and my own and behind this difference of ideas and method I believe there is precisely the difference of attitude of mind to which I have already referred. While Drzewiecki and myself are at one in dealing with the concentric annular elements of the propeller race as analogous to vertical strata of air in the case of the aerofoil, I believe I am right in saying that I differ from him in permitting or recognising the necessary inter-action between the fluid in the different annular elements in the case of the screw propeller, or the different vertical strata in the case of the aerofoil, by assigning properties to the blade depending upon its aspect ratio. In other words, the behaviour of the given element of a blade, and the value of that element as a unit of propulsion, can, according to my method, only be assessed when the aspect ratio of the whole blade is known and taken into account. The point is a rather subtle one, but in my opinion is of no little importance. Whatever the similarity or dissimilarity of my own methods and conclusions and those of Drzewiecki may be, I do not think I can be seriously blamed for having failed to make reference to work

with which I had no means of becoming acquainted and which, when I came to know of the existence, I found it so difficult to obtain information.

I have one further remark to make on this subject. There was a paper published by someone who shall be nameless, who described the method of designing airscrews in vogue at the Royal Aircraft Factory and attributed it, doubtless in good faith, to Drzewiecki. Now in this paper and the description of the method certain terms were used, such as *load grading, linear grading* and *thrust grading,* terms which I myself coined to deal with my own method. They are there as the headings of articles § 208, §209 and § 213 of my "Aerodynamics." I am quite sure that these titles came from my own book; I will leave others to judge whether the method described by means of these terms was inspired from any other source. As I have said on a previous occasion, a man who steals another man's gold should take the elementary precaution of throwing away the purse. I have defended myself at some length because I have been attacked. I could say more but I have to remember the ironical notice that was said to have adorned the cage of an animal of long-suffering disposition: "Cet animal est très méchant, quand on l'attaque il se défend."

Referring now to the two papers which I have mentioned as having been lost sight of by Major Low. In my paper read before the Institution of Naval Architects I dealt with the Froude theory of propulsion to which Major Low makes reference in the early part of his paper. Parenthetically, I would point out that in dealing with the Froude conception of an "actuator" we are not dealing with the screw propeller at all; that the ultimate intention is to make use of the theory in connection with the screw propeller is fairly obvious, but the fact that the theory is not directly concerned with the screw propeller should be always borne in mind and made clear. Before the date of my own paper there had been certain doubts about the arguments presented in connection with the Froude actuator which I endeavoured, with some measure of success, to clear up. The theory was also extended to deal with possible losses of energy not contemplated in the regime but which certain writers believed might exist. At the conclusion of this paper examples were given of the application of the theory to the stationary screw propeller, the helicopter and the windmill. As is often the case, not the least important part of this paper is to be found in the discussion in which I had to reply to criticisms by Mr. R. E. Froude, the Hon. Sir Charles Parsons, Mr. Sydney W. Barnaby, Professor J. B. Henderson and others. I think if Major Low studies this paper carefully, and especially the discussion, he will have considerable doubts as to whether, as he states, Professor Henderson had the better of the theoretical discussion when he ventured to cross swords with Mr. R. E. Froude. My own conclusion, supported by careful argument, was that Professor Henderson failed to establish his objections.

Passing now to my paper of April, 1915 (Proceedings Inst. Automobile Engineers, Vol. IX., page 263), a rather intricate theoretical investigation is presented, the main results of which are summarised in graphic form, Fig. 23 opposite page 310. This diagram gives the means of calculating the optimum condition of a propeller for any given duty. This optimum condition not only requires a certain diameter but a certain pitch which, under the conditions assumed, gives pitch = 1.46 times the diameter. Further this diagram gives the solution of the best pitch to adopt to secure maximum efficiency for any restricted diameter where circumstances do not allow of the optimum condition being adopted. There is also a curve deduced giving the theoretical efficiency for any case chosen between the limits to which the diagram applies and beyond this there is further given the theoretically best number or permissible number of blades which may be employed without loss of efficiency. The above data, as given in this diagram, furnish very nearly all the information that a propeller designer requires and, where comparisons are possible, they do agree very closely with what experience has shown to be the

best. Now this agreement would not be any great achievement if it had been based on empirical or experimental determinations—to all intents and purposes, however, it is based on abstract theory.

It is quite true that the investigation in question does not determine the shape of the blade, but the rules of the determination of the shape of the blade are given in Chapter IX. of my "Aerodynamics," to which reference has already been made. Beyond this, although I do not subscribe to the view that some rash rule-of-thumb workers have expressed, namely, that a twisted bit of hoop iron is as good as any other screw propeller, it is certainly true that if the diameter and pitch are chosen as the best possible for any given service, any good-looking blade is very little worse than the best. The same fact applies to the aerofoil, if we are dealing with flight efficiency alone and not other considerations (such as slow alighting speed) if the span be properly chosen for any given aspect ratio and flight speed, the best and the worst produced by any experienced designer are very little different in performance. In my opinion *the only factor in the form of blade section* which really counts is the mean curvature, the assumption being that the mean curvature is "faired" by eye to a good streamline form. It is this mean curvature which is dwelt on as the factor of importance in any aerofoil section, both in my "Aerodynamics" and in my paper on the aerofoil read before the Institution of Automobile Engineers in March, 1915 (Proceedings, Institution of Automobile Engineers, Vol. IX., page 171). The practical limitation in the screw propeller is the need for strength of blade, and thus it is not possible in a marine propeller blade to adopt so high an aspect ratio as in an air propeller; in the marine propeller a high aspect ratio blade will carry too great a thickness of body to give streamline flow. It is in this that the designer of an air propeller has the advantage.

I note that Major Low quotes Sir George Greenhill as having showed that if rotational velocity only be imparted by the blades to the column of fluid, this alone will account for the axial thrust on the screw. This is a result that ought not to pass unchallenged. If we are dealing with the regime contemplated by Rankine and Froude it is a patent absurdity; if on the other hand it merely means that we deal with the whole volume of fluid and not merely upon the propeller race then it is quite irrelevant to the investigation. It is equally irrelevant if the meaning is that the backward momentum communicated by the screw is equal and opposite to the forward momentum communicated to the water by the ship or vessel because the combination of vessel and propeller is not under discussion.

Mr. A. S. D. Poxcox (*contributed*): Major Low asked me to prepare the diagram of Fig. 7 for his paper and showed me the method of Fig. 6. I have no equipment for reading a book like Lamb's Hydrodynamics, but have no difficulty in seeing the graphical method as an application of the triangle of velocities, and the drawing of the curves in Fig. 7 was carried out by myself after a few minutes discussion of the method. It seems to me that circulation is very difficult to understand as a mathematical expression, but graphically very easy to see as the flow round the body, got by subtracting graphically the flow without lift from the flow with lift. There are always two diagonals that may be joined in each small parallelogram formed by the two sets of lines crossing; one gives the result of adding, the other the result of subtracting. If there is a circulation component of the flow, the circulation curves will be closed and will go round the body. With this rule it is easy to choose the right diagonals to join up in getting the new curves.

Major Low (*reply to written contributions*): With regard to Mr. Lanchester's protest, the supposition about failure to give reasons and references was confined to the circulation theory and was an attempt to explain the inexplicable neglect

of what the writer considers the most far-reaching advance in hydrodynamical theory since Helmholz's vortex theory. The writer cannot understand how he failed to see at once, some fourteen years ago now, that the key to the fascinating problem of flight had been found, save on the ground that it was not yet capable of yielding numerical values, which were demanded insistently by designers who had to get aeroplanes to fly. The application of the Biot-Savart law in electro-magnetic theory to the analogous problem in hydrodynamics has made Lanchester's method a numerical one, leaving only the circulation to be determined by experiment. This missing link in Lanchester's chain of reasoning has been filled in by the labours of the Prandtl school.

In the development of the blade element theory, Mr. Lanchester's independence of Drzewiecki is patent, his analysis of the problem is in many ways more complete and satisfactory, but here again there is a gap which the latter filled by experimental determination of his mean blade coefficients, while Lanchester claims to have obtained his more general coefficients by "good straight thinking," in Colonel O'Gorman's phrase, in which claim the writer is not yet prepared to follow him.

With regard to the Froude-Henderson controversy, it seems more natural to consider the wake in a finite basin as gradually opening out again and returning the energy of flow to the front of the actuator, less the amount dissipated by friction, than to maintain the column with unaltered diameter up to infinity, and there put it down a sink of unfillable capacity. The Goettingen type of tunnel was probably designed to conserve energy. In a dock the return flow is visible. In the N.P.L. type the conservation of energy is understood to be negligible. So that in the North Atlantic dissipation of energy must be very complete. Thus, Froude's assumption leads by an artificial line to the correct result, while Henderson's conclusion from a more natural line of thought is negatived by the rapid dissipation of energy.

Proceeding with the circulation theory, Mr. Glauert and Mr. McKinnon Wood seem to take as the fundamental case the limiting case of a disc covered with radial lines of vorticity, together the system of trailing vortex lines covering the surface of a cylindrical column of outflow uniformly. To this must be added the central axial core of vorticity. Prandtl, in the paper referred to, points out the need for adding half the axial inflow and subtracting half the rotary inflow, and as all corrections are applied to coefficients for infinite aspect ratio, there seems little left for Mr. Glauert to claim.

From the ideal limiting case, an attempt is made to argue back to the more practically important cases of screws with two, three or four blades. This seems to the writer to suffer from exactly the same defects as the attempt to argue from the Froude actuator disc back to bladed screws. The most direct way is obviously to calculate the effect of the open spiral of vortex lines trailing from the tips in conjunction with the complementary central core of vorticity and the circulation round the blades by evaluating the integrals in the appendix.

Self-interference is defined as the effect on a blade of the vortices streaming from its own tip and root, mutual interference as the effect on a blade of the circulation and vortex systems of the other blades, on the analogy of self and mutual induction of circuits in electro-magnetic theory.

For two blades the mutual interference is a small part of the total interference, for three blades not so small, for four blades still larger, and finally for the limiting case self-interference is negligible, and the ideal correction factor of one half the inflow component of velocity, additive for axial flow and subtractive for rotary flow, is reached, but it is to be applied to the coefficients of each element for infinite aspect ratio.

Thus, the difference of view is seen to be largely apparent and is completely reconciled by the correct application of the general theory.

Mr. Fage remarks that the reports criticised have been since superseded. This is to some extent inevitable as one can only criticise what has been openly published. It is satisfactory to know that the high standard of accuracy of measurement established at the N.P.L. is to be applied to the experimental examination of the circulation theory.

Mr. Lock's support of the general theory is most satisfactory, his criticism of detail being largely met by the reply to Mr. McKinnon Wood. The writer has qualified as "brilliant" the whole original work of a galaxy of able writers during a period of twenty years, Mr. Lock an expository pamphlet of twenty pages. Surely there is some disproportion in values.

Professor Bairstow approaches the subject from the consideration of Stoke's general equation of viscous flow. Apparently these equations have not yet yielded in his hands a demonstration of the existence of circulation, and until they do so he is cautious about accepting mere experimental evidence. The analogy he draws with elastic theory in a special problem in which the cyclic stress terms vanish, is weakened by the theory of dislocations of which a summary will be found in last year's B.A. reports. His objection to molecular theory should likewise be modified by the fact that apparent anomalies in the elastic behaviour of materials were recently explained by Professor Jenkin to this Society by means of a molecular model.

If Stoke's equations prove sufficient to account for the formation of eddies at solid boundaries then the writer's remarks will have to be modified. If not, then further coefficients will be required and molecular theory may be called in to explain their nature.

The writer is preparing for the Journal a brief account of the Kutta-Joukowsky transformations which will meet certain questions raised by Mr. Kirdany, by Mr. Zahra, and by Professor Bairstow himself. There is always a characteristic point at the tail which may be selected as point of stagnation even when the tail is rounded. The assumption is in the first place purely arbitrary, but Prandtl has offered a working hypothesis to justify it, and the close agreement in form with the observed distribution of velocity, gives it great interest and importance.

From elementary geometrical considerations it may be shown that the flow due to circulation alone is $K/2\pi a$, and that the flow due to the flow without circulation is $-2U \sin \alpha$, at any point on the circumference, α being the angle between the radius on which the point lies, and the velocity U at infinity.

Hence, having determined U and having assumed α arbitrarily, the value of the circulation is given by the relation $K/2\pi a = 2U \sin \alpha$.

In Fig. 7 α is about 60°, a value much beyond stalling point, and $K/2\pi a = \sqrt{3}\,U$. With a greater value than $K = 2\pi a U$ the stagnation point must be at a distance from the circle and from the profile, as is possible with a golf ball spinning fast enough, but not with an aerofoil.

With regard to the prediction of the stalling incidence, the writer has suggested a point of view which makes the stalling depend on the breakdown of the equilibrium of a Helmholz sheet of separation regarded as a vortex sheet and which may lead in the direction of a numerical determination. ("Engineering," 15th Dec., 1922, p. 739.)

In conclusion the criticisms called forth have been of a most stimulating nature and have greatly enhanced the value of anything that has been done in the paper itself to bring the Lanchester-Prandtl theory to the consideration of this Society and through it of the English-speaking aeronautical world.

AN ACOUSTIC TENSION METER

BY A. H. STUART, B.SC.

The demand for an instrument capable of measuring the load carried by a bracing wire has resulted in the designing of a number of tension meters. The majority of existing instruments make contact with the wire at three points and the force required to displace the middle point a definite amount is determined. Under favourable conditions the tension in the wire is a function of this force.

The chief objection to this class of instrument is that the application of the meter causes a change in length of the wire, and if the bracing wire be a short one, this change in length materially affects the tension. Much difficulty is also experienced with stiff wires.

Judgment of the tension in a wire by touch is very unreliable, and in bracing a structure without instrumental aid there is a very marked tendency to place an excessive stress on the bracing wires. There are many cases on record where the compression ribs of an aeroplane wing have failed during flight through a too great initial stress being put on the bracing wires.

The tension meter described below has enabled the writer to demonstrate this tendency to overstressing in many instances. The instrument has been used with marked success for checking the tensions in the internal bracing wires of aeroplane wings and fuselages. It has also been of much service in experimental work on a laboratory model of a Warren girder and similar structures.

The instrument depends upon the principle that the frequency of the note emitted by a wire is a function of (among other things) its tension. It consists essentially of two bridges, the distance between which can be varied from 20cms. to 40cms. These are placed in contact with the wire under test and the distance adjusted until the note emitted when the wire is plucked between the bridges is in unison with that given by a standard steel reed attached to the instrument. A suitable frequency for this reed is 276 vibrations per second; this is the "middle C" of modern concert pitch. A second reed of frequency 390 vibrations per second was added, however, to increase the range of the instrument.

Fig. 1 shows the general arrangement of the instrument. It is convenient to have a sound box between the bridges, and this may be connected by means of a rubber to some ear attachment such as a stethoscope for use when there is much external noise, as in a workshop.

The calibration of this instrument must be made experimentally. The well-known relation between the frequency n and the tension T expressed by

$$n = (1/2l)\sqrt{(T/m)}$$

is only true for what is generally called a "stretched string," that is, one in which the product of the modulus of elasticity of the material and the moment of inertia of the section is negligibly small. As this product is far from negligible in the wires used for bracing and the actual relation between n and T for stiff wires is unusually complicated, the experimental method of calibration is the only satisfactory procedure.

Figs. 2, 3 and 4 show the calibration graphs for steel wires known as "piano wire" of S.W.G. Nos. 20, 17 and 11 respectively. The following table gives particulars of these wires:—

S.W.G.	Diam. in ins.	Sectional area in sq. ins.	Weight per 100yds. in lbs.
20	0.036	0.00102	1.06
17	0.056	0.00246	2.54
11	0.116	0.0106	10.4

Fig. 5 shows the calibration graph for $\frac{1}{4}$in. B.S.F. streamline wire. This is a much heavier type of wire. It is swaged to a section of more or less streamline form and is equivalent to rod of 0.2in. diameter and weighs $32\frac{1}{2}$ lbs. per 100 yards.

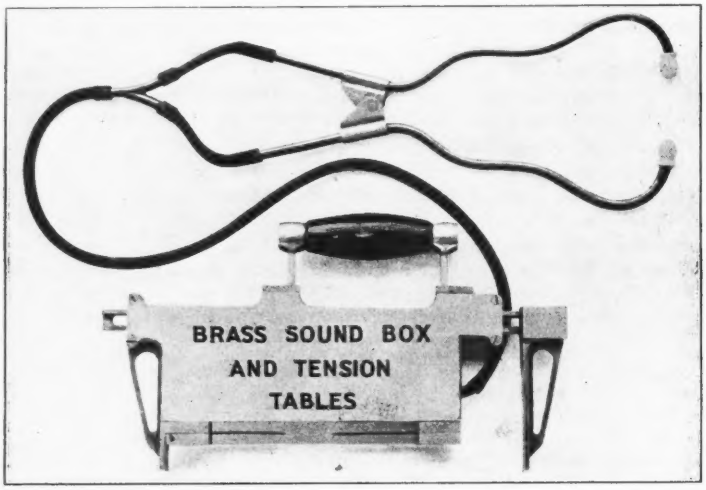

Fig. 1.—*Brass Sound Box and Tension Tables.*

An examination of these graphs shows that the readings are very consistent. It will be observed that some of the graphs are straight lines, while others are curves. It can be shown, however, that the calibration graph for any steel

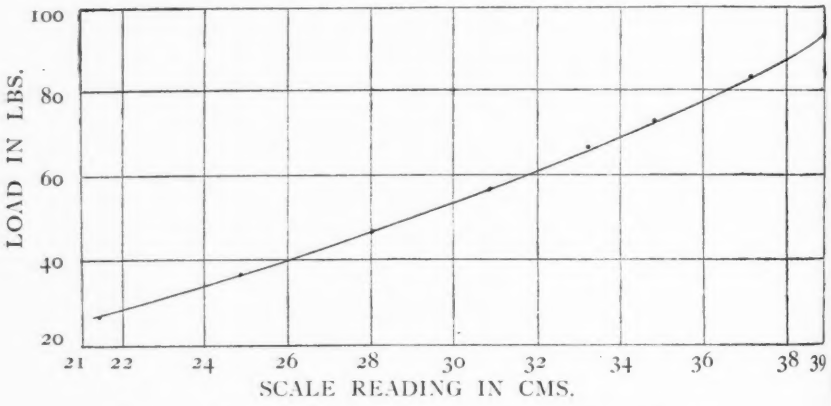

Fig. 2.—*Calibration Graph for No. 20 Piano Wire* $N = 390$.

wire does not sensibly deviate from a straight line so long as the stress does not exceed 20 tons per square inch.

Figs. 3 and 4 show the advantage derived from the addition of a second reed to the instrument. When the limit of the instrument is reached on the

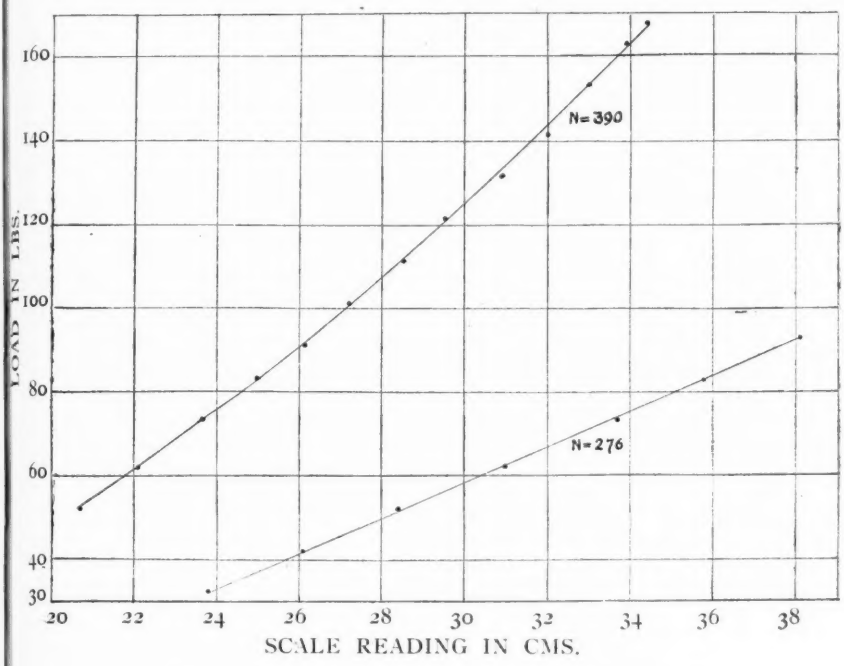

FIG. 3.—*Calibration Graph for No. 17 Piano Wire.*

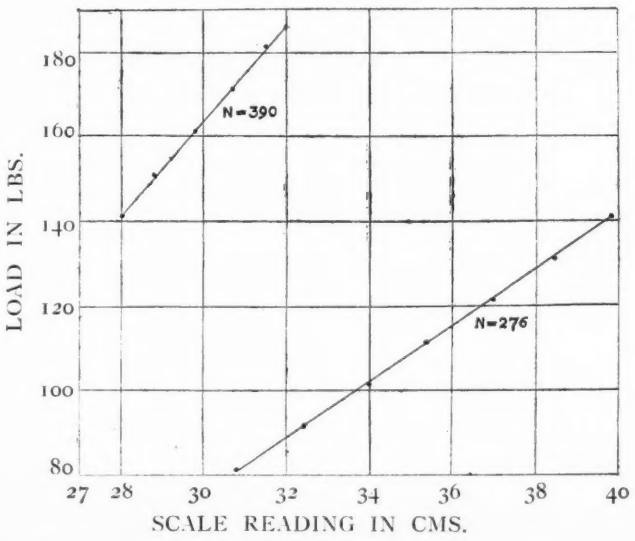

FIG. 4.—*Calibration Graph for No. 11 Piano Wire.*

reed of lower frequency, that of the higher frequency may be used. This makes the instrument available for loads approximately double those which may be determined by means of the reed of lower frequency.

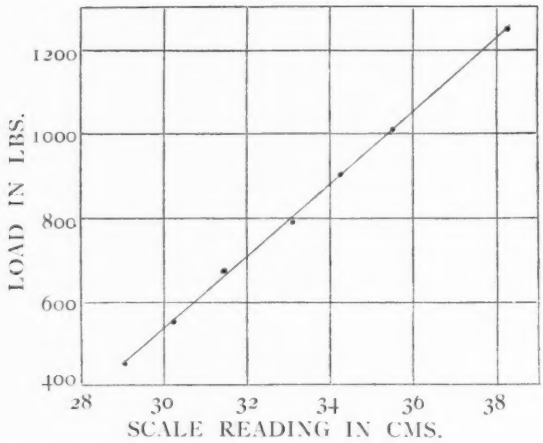

FIG. 5.—*Calibration Graph of* $\frac{1}{4}$ *B.S.F. Streamline Wire.* $N = 276$.

No particular skill or practice is required to use the instrument on wire of gauge up to S.W.G. No. 10. Generally speaking, and within reasonable limits, the thinner the wire and the higher the load, the more easily is the reading obtained. Nevertheless, the instrument *may* be applied to heavy wires under moderate stress, as Fig. 5 shows.

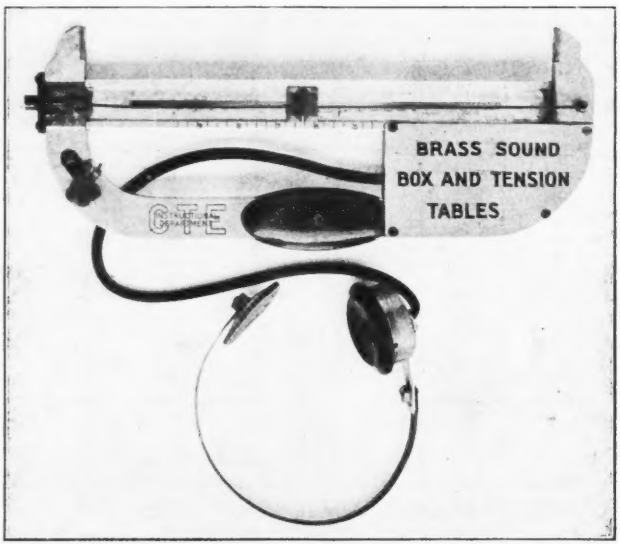

FIG. 6.—*Brass Sound Box and Tension Tables.*

The suitability of the instrument for general use was demonstrated in the following manner :—A length of No. 20 S.W.G. wire was loaded with 57 lbs. A section of eighteen boys of average age just over 16 made the test individually without any consultation or supervision whatever. The scale reading (which is all they were asked to find) for this load should be 30.8cms. (see Fig. 1). Of the eighteen readings obtained, twelve were excellent, and the average for the eighteen was 30.65cms. None of these boys had handled the instrument before, nor had they done any experimental work in sound.

An indication of the suitability of the instrument for experimental work is afforded by the fact that it was used exclusively for obtaining the data discussed in "The Internal Bracing of Aeroplane Wings" published in "Engineering" for August 26th, 1921.

APPENDIX.

The following tables give the actual experimental results from which the calibration graphs were drawn.

No. 20 S.W.G.
N = 390

Load in lbs.	Scale Reading.
27	21.4
37	24.8
47	28.0
57	30.8
67	33.2
73¼	34.8
83¼	37.1
93¼	39.0

No. 11 S.W.G.
N = 276

Load in lbs.	Scale Reading.
81¼	30.8
91¼	32.4
101¼	34.0
111¼	35.4
121¼	37.0
131¼	38.5
141¼	39.9

N = 390

141¼	28.0
151¼	28.8
161¼	29.8
171¼	30.7
181¼	31.5
186¼	32.0

No. 17 S.W.G.
N = 276

Load in lbs.	Scale Reading.
32	23.8
42	26.1
52	28.4
62	31.0
73¼	33.7
83¼	35.8
93¼	38.1

N = 390

52	20.7
62	22.1
73¼	23.6
83¼	25.0
91¼	26.1
101¼	27.2
111¼	28.5
121¼	29.5
131¼	30.9
141¼	32.0
153¼	33.0
163¼	33.9
168¼	34.4

¼in. B.S.F. Streamline.
N = 276

Load in lbs.	Scale Reading.
450	29.0
550	30.2
675	31.4
790	33.1
900	34.2
1010	35.5
1245	38.25

It may be of interest to add that before this instrument was designed, one of the form shown in Fig. 6 was made. In this example the distance between the bridges was fixed (12ins.) and the frequency of the comparison note was varied.

The latter variation was accomplished by providing a suitable "loaded" wire, the tension of which was adjusted until six inches of it gave a note of frequency 276. A bridge moving under this wire and in contact with a scale was the means provided for adjusting the frequency of the comparison note until it was in unison with that emitted by the wire under test.

This instrument was very satisfactory in use, its only disadvantage being that it required "tuning up" before a test was commenced and it was necessary to check this adjustment at intervals during the test. This disadvantage was felt to be very grave and the instrument was discarded in spite of the simplicity of its construction.

REVIEWS

Steel Thermal Treatment

By John W. Urquhart. Publishers: Crosby, Lockwood and Son. Price 35s. net.

This book is an unsatisfactory book from many points of view and is not to be recommended. Data has been gathered from various sources and used with little discrimination, with the result that it contains information of a misleading kind and in part is self-contradictory. As an instance, in the early definitions, annealing and normalising are taken as one operation, but are later shown to be quite distinct treatments. A number of definitions might be severely criticised. Some of the furnace descriptions are good, but the matter relative to the treatment of steel gives such data as, for instance, that the main changes in a 1 per cent. carbon steel were at 1,450°F. and 760°C. on heating and cooling respectively, which is quite inaccurate. The compilation of physical tests is weak and unsatisfactory.

Aircraft Steels and Material

By Brig.-Gen. R. K. Bagnall-Wild, C.M.G., C.B.E., Leslie Aitchison, D.Met., B.Sc., A.I.C., A. A. Remington, O.B.E., M.I.Mech.E., M.I.A.E., A. J. Rowledge, W. A. Thain, A.M.Inst.C.E. With an introduction by Professor W. Ripper, C.H., D.Eng., D.Sc., M.Inst.C.E. Publishers: Constable and Co., Ltd. Price 16s.

This book, embodying the lectures delivered on behalf of the Society at Sheffield University in 1921, is a useful volume to have on one's shelf as indicating the standard of technique of aircraft steels during the war. To the engineer it will be found to contain much information and data which will teach him the necessity for thoroughly understanding the characteristics of the steels which he employs.

Certain matters touched upon in the book are still of a controversial nature and most of the conclusions drawn can by no means be considered as final. One may mention, for example, the reference to hot twisted crankshafts on page 25. One would conclude from the figures here given that twisting was a most damaging process, whilst those familiar with it and with the test results will know that the figures given are misleading and unrepresentative, and hence the conclusions are not satisfactory. Hot twisting was largely used for the crankshafts during the war and is still used very successfully.

The lecture on "Cold Worked Steels" contains a number of anomalous statements, for instance, "Theoretically, it should be possible to obtain any tensile strength in a steel which has been cold worked." What "theory" is here invoked it will be interesting to know. In comparing soft and hard steels for cold working capacity it is stated that "the ratio of original and final strength is constant just as the proportion of cold working is constant." Those familiar with tests on such material know that mild steel is capable of much more deformation as well as of greater proportionate increase in tensile strength than some harder varieties, although perhaps the final strength may be somewhat inferior. The "variation in modulus figures" obtained is not really large enough to be of practical importance. Moreover, the low figures given for untempered cold worked materials are largely open to question on account of the masking of the real figures due to internal stresses left in the material after the cold working operation. In referring to the effect of inclusions on cold working, the author states that the elongation these receive by hot working may be further extended by cold working. Surely the proper view is that the inclusions are harmful in such materials since they break instead of elongating and thus form discontinuities of material.

The other lectures do not call for special comment. The notes on case-hardening and the hints to practical designers are very suggestive. There is, unfortunately, a certain amount of repetition and overlapping of the different sections, but perhaps this is unavoidable in a book of this kind where the sections are contributed by different writers.

The publication is open to one serious criticism. The illustrations of aircraft machines and engines given are generally so small as to be of little use, whilst the micro photographs used throughout the book are generally given without any indication of the magnification employed.

The book may be valuable as a record of certain metallurgical features of the war period.

THE CASE FOR METAL CONSTRUCTION

Tables Omitted from January Issue (Pages 5 and 6).

TABLE I.

Material.	E Tons/□″.	Sp. Gr.	E/S.G.
Spruce, Spec. 2 VI.	670	.45	1490
Steel	13,000	7.8	1667
Duralumin Aluminium Alloy, Spec. 2 L.3	4,790	2.85	1680
Magnesium Alloys	2,400	1.75	1370

TABLE II.

Material.	Identification Composition.	Critical Stress.	Specific Gravity.	Critical Stress. Specific Gravity.
Spruce	Spec. 2 VI.	2.01	0.45	4.47
Hard Rolled Alum Strip	L.4 — Al. 98% ...	8	2.6	3.08
Al. Alloy Strip Duralumin	Spec. 2 L.3. Cu. ... ... 4.0% Mn. ... ... 0.5% Mg. 0.5%	13.5	2.85	4.74
H.T. Alum Alloy ...	L.29	19	3.15	6
Mild Steel	Spec. S.3. C. 0.2% Normalised	18	7.8	2.31
Plain Carbon	C. 0.1% Cold Rolled	28	7.8	3.59
	C. 0.1% Blued ...	24	7.8	3.08
B. and P. Test Results	Spec. S.44 C. 0.4% — Blued ...	35	7.8	4.49
Alloy Steel Strip ...	Spec. S.43. C. 2% Mn. 0.4% Ni. 3.5% Cr. 1.0% Va. 0.1%	40	7.8	5.13
Hardened and Tempered	Spec. S.40. C. 0.3% Mn. 0.4% Ni. 4.25% Cr. 1.0% Va. 0.1%	65	7.8	8.33

NOTE.—Except where otherwise stated the Figs. are for B.E.S.A. Specification.